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TO OUR READERS AND CORRESPONDENTS.

We are much obliged by *Cantab's* letter, and will look more sharply into those matters in future.

At page 345, line 12, of our last volume, for "invariably," *read* "inversely."

"An old Subscriber," who accuses us of "puffing," had better not stir the subject of his letter, least we should communicate information which might be very disagreeable to some of the gentlemen whose cause we presume he intends to advocate. No person or thing has ever been puffed in this Journal.

The suggestions of "Medicus" respecting the New Pharmacopœia, shall be attended to.

Sir George Mackenzie's paper has duly reached us; we regret that want of room obliges us to postpone its publication.

The article on the Mechanic's Institution is too long for insertion.

We are sorry again to postpone our Correspondent γ. β.

The letter dated "Glasgow, March 1," shall have due consideration.

Among *Miscellaneous Intelligence* our readers will find much that should have been placed under the head of *Foreign Science.*

We have received many communications and inquiries respecting the Royal Institution, and feel much obliged for the facts which several of them contain. The Visiter's Report will shortly be laid before the Members, and will probably include the information sought for by some of our Correspondents; if not, they shall be replied to in our next.

The observations of F. R. S., and those contained in a letter dated "Kensington," and in another signed "Amicus Justitiæ," are extremely pertinent and judicious, but we feel disinclined to enter into the subjects they touch upon *if we can possibly avoid it.*

The parcel from Manchester has just arrived. (March 29.)

THE

QUARTERLY JOURNAL,

April, 1821.

ART. I. *On some Phenomena relating to the Formation of Dew on Metallic Surfaces. By* George Harvey, M G. S., *&c. &c.*

[Communicated by the Author.]

IT is a curious fact, mentioned by Dr. Wells, in his valuable *Essay on Dew*, that if a metallic substance be closely attached to a body of some thickness, which attracts dew powerfully, the tendency of the metal to promote the formation of moisture on its surface, instead of being increased from the circumstance, is diminished, provided the metal covers the whole of the upper surface of the body to which it is attached*. This principle he illustrated by the following experiment: Two pieces of very light wood, each four inches long, a third of an inch wide, and one tenth of an inch thick, were joined in the form of a cross; and to one of its sides the non-metallic surface of a square piece of gilt paper was attached, by means of mucilage. On exposing the metallic surface on a dewy night, by suspending it in a horizontal position, about six inches above the ground, he found after a few hours, that the parts of the metallic paper, not in contact with the wood, had minute drops of dew on their surfaces, while those in contact with the cross, were perfectly dry.

In repeating this experiment, I have employed gold and silver

* Wells on Dew, page 22, second edition.

metallic paper, attached to frames of various forms; and by prosecuting the subject under different circumstances of the atmosphere, I have met with some interesting and beautiful phenomena, which seem to merit a particular description.

The metallic squares were sometimes suspended a few inches above the ground, and at other times placed on surfaces of glass, or on the recently-mowed herbage. The particular situation of each will however be noticed, as the different experiments are described.

In endeavouring to trace phenomena relating to the deposition of dew on the surfaces of polished metals, some perseverance is necessary; as it is but seldom that the circumstances of temperature and moisture are such, as to permit its ready formation. It would appear, that not only the depression of temperature, and the presence of moisture in the lowest atmospheric stratum, must be considerable; but that the superficial dimensions of the metal have also an influence on the formation of moisture on it. The difference between glass and polished metals in this particular is singularly remarkable. A small vitreous surface, when presented to a clear and tranquil sky, has its surface as readily covered with moisture as one of larger dimensions; but in the case of metals of the same kind, of polished tin for example, a large metallic plate is sometimes more readily dewed than a small one; whereas, under other circumstances, one of a small area is covered with a copious deposition of moisture, whilst a large one will preserve, during the whole night, a bright and unsullied surface*. I have thought it proper to introduce these remarks, in order to apprize the young inquirer of the disappointment to which he will be frequently liable, when prosecuting this interesting subject with relation to polished metals.

Whenever the squares of silver paper were exposed for the pur-

* "A large metallic plate, lying on grass, resists the formation of dew more powerfully than a very small one similarly situated. If a large and a very small plate be suspended horizontally, at the same height in the air, the small plate will resist the formation of dew more powerfully than the large."—WELLS on *Dew, page 22, second edition.*

pose of receiving dew, it was remarked, that the first formation of moisture took place at the corners of the triangular portions of the metallic paper, not in contact with the wood; the particles being exceedingly minute, and requiring the aid of a magnifying glass to discover them. As the radiation of the metallic surfaces was promoted by the influence of the clear nocturnal sky, those particles of moisture gradually increased both in number and size; while other minute drops began at the same time to be deposited on the edges of the square; so that in the course of three hours, the metallic surface had assumed the appearance represented in fig. 1, Plate I. the shaded parts denoting the particles of dew, and the dotted lines the position of the cross to which the metallic paper was attached. After midnight, the farther deposition of moisture appears to have been suspended; as at half an hour before sunrise, the appearance of the metal was nearly the same as when the last observation was made.

It was most interesting to observe, during the progressive deposition of the moisture, that the particles were disposed in triangular forms, similar to the right-angled triangles, into which the metallic paper was divided, by its contact with the cross; and this was the case even when the triangles, from their minuteness, might be esteemed of an almost elementary kind. And at the last observation, when the greatest quantity of dew for the night had been deposited, the triangular figures were perfectly well defined, their hypothenuses being bounded by the edges of the metallic surface, and their several bases and perpendiculars, respectively parallel to the arms of the cross. So also the gradual accumulation of moisture in the small segments, whose chords coincided with the edges of the metal, was marked by the same uniform and progressive character; the particles, during their increase in number and magnitude, preserving a beautiful curvilineal contour to the figures which they formed. The parts of the paper in contact with the cross, had no dewy particles on them; their junction with the wood appearing effectually to prevent the formation of moisture; thus confirming the observation of Dr. Wells.

On another night, favourable to the copious formation of dew,

the moisture was not confined to the small triangular and curvilineal spaces alluded to in the preceding experiment, but was diffused over the whole surface, excepting the parts in contact with the wood. The metallic surface presented therefore a dry portion with well defined borders, in the form of the algebraic sign *plus*; and four triangles of dew, formed of particles beautifully distinct, but undergoing a minute diminution in size, from the edges of the paper to the vertices of the triangles, as represented in fig. 2. To contemplate these triangular formations of dew to advantage, it was necessary to place the eye in a situation to receive the impression of the reflected light. In such a position the forms of the triangles were viewed to great advantage; the innumerable atoms of dew presenting a pleasing contrast to the unsullied figure of the cross.

On the same night a similar surface of gold metallic paper, similarly circumstanced, presented an appearance as in fig. 3, in some degree analagous to that represented for the silver paper in fig. 1. The moisture seemed however destitute of that uniformity which characterized the particles formed in the last-mentioned diagram, although some slight approach to it might be traced, in the formation of the irregular patches of dew, in the angular portions of the figure.

On the following night, portions of silvered paper attached to triangular and square frames, as denoted by figures 4 and 5, were presented to the nocturnal sky. In the former, the beautifully minute particles of dew were confined to the equilateral triangular surface, not in contact with the frame; the particles however seeming to preserve an uniformity of magnitude over the whole surface. To the middle of the non-metallic surface of fig. 5, a circular piece of wood was attached, of the same thickness as the frame; and during the abundant deposition of dew which took place in the course of the night, the moisture was strictly confined to the parts of the metallic paper, not in contact with the wood; the small circular portion, although surrounded as it were with an atmosphere of moisture, presenting as effectual a barrier to its formation as the external frame.

During the prosecution of these experiments, I have had frequent opportunities of remarking that silver metallic paper permits dew to be deposited earlier, and in greater abundance on its surface, than gold. Early in the month of April, at nine P. M., a large pane of glass was placed on the green herbage, and on it the squares of gold and silver paper attached to their respective crosses. The clear and transparent sky, joined to the perfectly tranquil state of the atmosphere, indicated the possibility of a copious deposition of dew. At six the next morning the grass exhibited the appearance of a thick hoar frost, and the moisture which had been formed on the upper and under sides of the glass during the night, presented coats of transparent ice. On referring to the squares of metallic paper, that of gold was found removed from the glass on which it had been placed the preceding evening, to the distance of six feet; its change of situation having been probably produced by the force of some breeze during the night. The metallic side was in contact with the grass, and on taking it up it presented four beautiful triangles, completely covered with innumerable particles of frozen dew. Those parts of the metal which had their inferior surfaces in contact with the wood, exhibited the perfect and well defined form of the cross, represented in fig. 6. The appearance of the crystalline triangles, when contrasted with the golden surface ef the cross, was extremely beautiful; and it was remarked, that as the gradually increased warmth of the morning dissolved the crystals of dew, the moisture was still confined to the same triangular surfaces: thus preserving completely the form of the cross. Minute crystalline atoms were also perceptible on the non-metallic side of the paper, and which, likewise dissolving, had a sensible effect on the rigidity of the paper. On examining the silver square which had preserved its situation on the glass, its metallic surface was found without the appearance of moisture, under any form, on its surface.

A hasty consideration of this phenomenon might lead us to infer, that dew is more readily deposited on gold than on silver, contrary to what has been before remarked, as the result of extended

observations; but a farther investigation of the anomaly in question may lead to a satisfactory explanation of its cause.

The appearance of the heavens at the time the metallic surfaces were placed in the meadow for observation, indicated, as before remarked, the probability of a copious deposition of dew, during the night, and that a considerable quantity was deposited, the hoar-frost in the morning clearly proved. The temperature, and the hygrometric state of the air, were also, from other collateral circumstances, to be regarded as highly favourable to the formation of dew on metallic bodies; and that a breeze must have existed for some time during the night, sufficiently powerful, at least, to remove the metallic square, together with its attached cross, to the distance of several feet, is likewise apparent. These circumstances will account for the anomaly in question.

In the first place, dew was most probably deposited during the former part of the night, in a sufficiently copious degree, to cover the four triangles on each of the metallic surfaces. This deposition may be presumed to have taken place before the temperature of the lowest stratum of air, in contact with those surfaces, was depressed to that of the freezing point. The breeze removed the golden square, and left its metallic surface in contact with the short herbage, the temperature of which had been previously reduced to 32°. This temperature necessarily caused the particles of dew already deposited on the triangular surfaces to crystallize; and left the cross with its lustre undiminished. The same wind dissipated the moisture that had been deposited on the silver surface; for it has been remarked by Dr. Wells*, that "the dew which has formed upon a metal will often disappear, while other substances in their neighbourhood remain wet." The breeze indeed may have continued the remainder of the night, and prevented any new formation of dew on the silvery surface; but, at the same time, permitted moisture to be deposited on the non-metallic surface of the golden square; because white paper has been placed by

* Page 21, *Essay on Dew*, by Dr. Wells, second edition.

the last-mentioned philosopher among the substances that are even more productive of cold than wool*. Or it is possible that the breeze may have subsided, and the circumstances of temperature become such as to have allowed the deposition of dew on the paper, but not of its re-formation on the silver.

Dr. Wells has also remarked †, that when dew forms upon metals, it "commonly sullies only the lustre of their surface; and that even when it is sufficiently abundant to gather into drops, they are almost always small and distinct." This observation, however, requires some limitation; since, on nights that have been more than usually cold, and when the quantity of moisture in the air has been abundant, I have observed the dewy particles deposited on metals to attain a considerable magnitude; and examples have even occurred of polished tin surfaces being completely covered with thin sheets of water, the result of the junction of the innumerable minute particles deposited on them.

On one night, equal squares (their linear edges being one inch and half) of lead, zinc, brass, copper, and tin, were laid on a large plate of glass, and presented to the influence of a clear sky. At sunrise the next morning, the particles of dew on the different surfaces were found of variable magnitudes; those on the lead being the largest, and of the size represented in fig. 7. Those on the zinc were next in magnitude, as denoted in fig. 8; and the particles on the brass were still smaller, but much more numerous, as in fig. 9. The copper and tin, particularly the latter, seemed only to have had the lustre of their surfaces just dimmed, by the abundant moisture of the air. Lead, therefore, was at one extreme of the series, and tin at the other; brass holding a middle rank between the two.

This relation, however, between the particles on lead and brass, was inverted on another night, when equal squares were laid on the recently cut herbage, the particles on the brass being of the size represented in fig. 10, and those on the lead as denoted in fig. 11. As the plates of metal were the same in both cases, it is reasonable

* Page 21, *Essay on Dew*, by Dr. Wells, second edition.
† Page 21, second edition.

to infer that the opposite results observed were produced by the substances on which they were respectively placed. A slight trace of moisture was perceptible on the zinc, but not the least degree on the copper and tin.

An example of the slowness with which polished tin permits moisture to be deposited on it, *occurred when a concave mirror, formed of polished block tin*, was employed as an Æthrioscope, on the plan first suggested by Dr. Wollaston. The focus of the instrument, at the time the experiment was performed, was 20 inches above the ground. The night was tranquil, and dew was copiously deposited on glass, a few minutes after it was presented to the chilling influence of the transparent sky. At nine, P. M. the thermometer in the focus of the Æthrioscope indicated a temperature of 46° ; the herbage being at the same time 44° ; and the air, seven feet above the ground, 49½°. Observations, connected with some other phenomena, were made every half hour; but no trace of moisture was perceptible on the metallic surface, till two A. M., when it appeared slightly dimmed, although other substances had gained considerable increments of dew in the same time ; masses of wool, for example, having increased in weight from twelve grains to thirty. At the same moment, the focal thermometer indicated a temperature of 42½° ; that on the grass, 39° ; and that elevated in the air, 45°. In five hours, therefore, the cold of the upper sky only underwent a change of 3½° ; whereas the grass lost by radiation in the same time 5°; and the elevated stratum of air diminished its temperature 4½°. From two o'clock to three, the thermometer remained stationary, but the moisture had sensibly increased on the surface of the Æthrioscope, and increments amounting to several grains, were likewise found on other substances ; a proof, that if the general temperature remains stationary, *after* the temperature of a body is sufficiently lowered to permit the formation of dew on its surface, the farther deposition of moisture is not prevented. At four A. M. the whole metallic surface was covered with visible drops, the temperature, at the same moment (just before sunrise), indicating the maximum of cold, the focal thermometer

being at 40°; that on the grass, 37°; and that elevated seven feet above the ground, 41½°. It is worthy of remark, that two plain sheets of polished tin, placed horizontally on the herbage, had not the slightest trace of moisture on them.

On another night, however, when there was every prospect of an abundant deposition of dew, the influence of the grass in promoting its formation on metals, was clearly shown. At nine P. M. two plates of polished tin, one fourteen inches by ten, and the other six by two, were laid on very short grass. Another plate of the same dimensions as the former was placed gently on the long grass. Its weight necessarily compressed the herbage on which it rested, so that the polished surface was surrounded on all sides by grass, reaching twelve inches above it. In fig. 15 the long grass is represented on two opposite sides of the tin M N, together with the compressed herbage below it. At eighteen inches above the ground, or two inches above the average height of the grass, a similar plate, O P, was placed on slender props. The temperature of the grass at the moment the plates were exposed was 60°, and of the air 65°; being a difference of 5° in the small space of three feet. At five the next morning, a great quantity of dew was formed on the grass. A register thermometer on the short herbage, indicated the maximum cold to have been 52°, and of the air, at the elevation before mentioned, 60°. The difference between these maximum depressions of temperature was, therefore, by no means considerable; and the copious deposition of dew observed was to be regarded rather as the result of the abundance of moisture in the atmosphere, than as a consequence of great difference of temperature.

The metals presented the following particulars for observation. The plates resting on the short herbage had a few scattered patches of dew on their upper surfaces, but nothing like a regular and uniform deposition. The plate M N, surrounded by the long grass, had its superior surface completely covered with minute but distinct particles of moisture; but the plate O P, elevated above the grass, was perfectly dry. This difference in the results must be regarded as arising from the different conditions, under which the plates were situated. The latter surface, it will appear, had not its tem-

perature depressed *below* that of the stratum of air reposing on it, during the night; but the former must have been considerably colder than the column of air hovering above it. The cooling power of the grass surrounding the plate M N, and on which it also rested, must have necessarily extended its influence to the metal; and by lowering its temperature considerably, have occasioned the copious deposition observed. The upper plate not being in contact with the grass, permitted the air to pass freely on each side of it; and being itself a bad radiator, attained no condition during the night favourable to the deposition of dew. With respect to the formation of dew being less abundant on the plates resting on the short herbage, than on that surrounded with the long grass, it may, in one point of view, be regarded as a consequence of the curious fact observed by Mr. Six, that the temperature of short grass is always greater than that of long grass. The state of the herbage has always a considerable influence on the quantity of dew deposited, and the greater the body it presents, the more abundant it is likely will be the formation. That the *quantity* of herbage has a considerable effect, may be inferred from the experiment, that when one mass of wool was placed on short herbage, and another of equal size and weight on the summit of a mass of recently cut grass, fifty inches above the ground, the moisture gained by the former during the night, was only fifteen grains, whereas the increment to the latter was twenty-three.

In consequence of the plate O P having had its surface exposed to the entire canopy of the sky, but the view from the plate M N being confined to a comparatively small circular space, in the zenith of observation, it might be inferred from a principle adopted by Dr. Wells*, that the former would have gained more moisture than the latter. But the maxim of this ingenious philosopher is evidently limited to the consideration, that the bodies are in other respects similarly circumstanced. For instance, in one of the experiments

* *Essay on Dew*, page 14, second edition. The principle here alluded to is the following: "Whatever diminishes the view of the sky, as seen from the exposed body, occasions the quantity of dew which is formed upon it, to be *less* than would have occurred if the exposure to the sky had been complete."

instituted by Dr. Wells, to illustrate the principle in question, by bending a sheet of pasteboard into the form of the roof of a house, and placing it with its ridge uppermost, and ends open, over a mass of ten grains of wool laid on the grass; and at the same time placing another equal mass on the herbage, fully exposed to the sky, the former gained, during the night, an increment of only two grains, whereas the latter gained sixteen. In this experiment, the two masses were placed under the same circumstances, so far as contact with the grass was concerned; but in the case relative to the plates of tin, one was not only in contact with the herbage, but also surrounded by it; whereas the other was completely detached.

The gradual manner in which dew is deposited on the metallic side of gilded glass was pleasingly exemplified on another occasion. The parallelogram of glass was six inches by four, as represented in fig. 14. It was first exposed to the atmosphere with its metallic side uppermost, at half-past six, P. M., being about three quarters of an hour after sunset. The atmosphere was clear, and highly charged with moisture; and dew had formed on glass in a shady place, three quarters of an hour before the departure of the solar orb. A mild and gentle breeze prevailed also at the same time. No perceptible change took place in the metallic surface until eight, when minute particles of dew were visible at A, the leeward end. From the last-mentioned hour to ten, the moisture gradually increased from A to the middle part of the surface; and distinct drops were likewise deposited at D, B, E, C. As the particles increased in size round the three edges, other minute drops were successively deposited, more distant from them; and it was observed, that they accumulated with most rapidity at the leeward sides A and C. At eleven, P. M., when the sketch represented in the figure was made, an oval portion of the metallic surface was found entirely free from moisture. The same figure was also perfectly visible at midnight, when the drops at A had increased to at least an eighth of an inch in diameter; those at C being rather less. The particles at the corners D and E also preserved their superiority in size above those at B.

The difference in the appearance of dew, when deposited on tin and on glass, is sufficiently remarkable to arrest attention, not only when the moisture remains uncrystallized, but also when it is frozen. In an example that occurred of the latter case, a decrease in the magnitudes of the frozen particles could be traced from its edge to the dry and unfrozen margin surrounding a parcel of wool, placed on the middle of the plate, as represented in fig. 13: the appearance of the frozen atoms partaking, in some degree, of the lustre of the tin. The parcel of wool, in the interval from nine P. M. to midnight, gained four grains of moisture; and from the last mentioned hour, to six the next morning, thirty-two grains; thus gaining, in a double time, an eight fold quantity of moisture. The wool was frozen to the tin; and when the rays of the sun fell on the metallic surface, the crystalline particles became detached from it, and were readily collected together. The dew deposited on the glass presented an irregular fibrous appearance, its colour partaking of the greenish hue of the crystal. The icy particles on the tin were first deposited as dew, and frozen before they had collected in sufficient numbers to run into each other, and form an uniform crystalline surface. But the dew on the glass being formed at an earlier period of the night, a sufficient quantity was deposited to cause the particles to mingle with each other, and thus to present to the action of the freezing temperature a wide spread surface of water. The unequal action of the glass, combined with the law which regulates the crystallization of water, communicated to the frozen surface of dew, the fibrous and irregular character represented in fig. 12. Soon after the solar rays had impinged on the glass, filaments of ice were detached from both its surfaces, that from the upper side being much the thickest.

Plymouth, December 12, 1823.

Art. II. *Description of Two New and Remarkable Fresh Water Shells: Melania setosa and Unio gigas.* By William Swainson, Esq., F.R.S., L.S., M.W.S., *&c.*

[Communicated by the Author.]

The attention of several conchologists has been excited by a new and most extraordinary fluviatile shell, belonging to the genus *Melania*, recently brought from the Mauritius. Having been favoured with its examination, I now lay before the public the following description of the shell, drawn up from the only specimen which its discoverer, Mr. Warwick, was able to procure, after diligent and often repeated searches in the same locality. I take this opportunity also of recording the characters of another fresh-water shell of gigantic dimensions, equally unknown and interesting to naturalists.

Melania. Lam: Cuv.

Specific character.

M. testâ ovatâ, ventricosâ, spinis tubularibus seta bina porrecta basi connexa emittentibus coronatâ. Shell ovate, ventricose, coronated by tubular spines, each sheathing the base of two protruded horny bristles.

Description.

Length, one inch two tenths, of which the spire occupies very nearly one half. In *habit* the shell resembles *Melania amarula*, (Helix amarula, Lin.) but the basal volution is more ventricose, the spire more conic, and the tip acute; it is also much thinner, and may be termed subdiaphanous; the whole shell is covered by an olive brown epidermis; the spiral volutions are angulated, and marked by from three to four transverse elevated striæ; the basal volution is without any indication of plaits, but is slightly impressed by narrow, transverse grooves, which are wide apart; these are crossed by very delicate and close-set longitudinal striæ; but whether these last are only external and belong alone to the epidermis, could not be ascertained without injury to the specimen. The most extraordinary characteristic of this shell, I shall now proceed to detail. On the upper part (or shoulder as it is sometimes called,) of the body whorl, is a row of coronated spines,

perfectly tubular; these spines are very thin, and are placed parallel with and very near to the aperture; their summits are obtuse and their length variable, probably owing to some having been injured through their great delicacy; the longest measured nearly one-eighth of an inch; from the summit of each spine emerges two stiff erect acute bristles; closely adhering together, and projecting about two-tenths of an inch. The colour of these bristles is black, their surface polished, and their substance horny. They likewise possess some degree of elasticity, being easily bent by a slight pressure applied laterally; although I doubt whether they would have sustained such pressure had it been applied horizontally. These bristles it will be perceived, are completely sheathed at their base by the tubular spines, but these latter are so thin that the lower part of the bristles are distinctly seen through them; rooted, as it were, in the substance of the shell. I know not, positively, whether each spine contains *two* distinct bristles; or only one, forked or divided at about half its length, as this fact could only be ascertained by removing one of the spines, and tracing how far the division extended; but that portion which forms the lower half (and is enclosed within the spine) is so thick, as to favour the supposition of their being in pairs. These spines are continued round the middle of each volution of the spire to its apex; but they are more remote, and the bristles much shorter, than those on the body whorl; sometimes, indeed they hardly project beyond the spines. The direction of the whole is slightly incurved. The aperture is pale; and, at the top of the outer-lip, is an indented sinus similar to that seen in *M. amarula*, Lam.

Ob. 1. The extraordinary appearance of bristles protruding from the spines of a shell, a formation altogether unprecedented amongst this class of animals, might naturally excite, in some minds, a suspicion that it was an ingenious deception. But this idea, I think will be abandoned, when the peculiar construction of the spines are well considered. In the genus *voluta*, we have many instances of shells being crowned with thin, *vaulted* spines, but no example can be produced, of such coronated spines being *tubular;* or completely closed in their circumference, and pervious only at their

summits. Now it is obvious, that this peculiar form, is of all others the best adapted to strengthen and protect the elastic bristles which they enclose: both appendages, therefore, are in unison with each other, and leave not a doubt in my mind, (setting aside the personal testimony of its discoverer) that the whole shell is in a perfectly natural state.

It is difficult to conjecture in what way the formation of the shell accords with the economy of its inhabitant. We know that testaceous mollusca, are the food of several kinds of fish, both marine and fresh-water; may not these bristles be intended by nature to defend the animal from such enemies? they would certainly be very repulsive to the lips of any fish; and in all probability would penetrate, as deep as possible into the skin. The weapons of protection or of defence with which nature has furnished different tribes of animals, are as various as they are wonderful. In the testaceous mollusca, they are confined alone to the shelly covering of the animal, who, as long as his castle is armed and entire, withdraws into its walls, secures the entrance, and remains passively secure.

Ob. 2. Since the above was written, Mr. Broderip informs me, another specimen of this shell has come into his possession: "carefully cleared, and every vestige of bristle removed, the hollow coronations remain."

UNIO GIGAS.

Specific character.

U. testâ ovato-oblongâ, depressâ, anticè alatâ et sulcis obliquis, divaricatis subradiatâ; posticè brevissimâ; dente laterali (utriusque valvæ) solitario; umbonibus brevibus, retusis.

Shell ovate-oblong, depressed anterior side winged and marked by oblique grooves in different directions; posterior side very short; lateral teeth, one in each valve; umbones small, retuse.

DESCRIPTION.

This is truly a gigantic shell; far exceeding in size any other yet discovered as inhabiting the fresh-water, and presenting characters which leave no doubt that it has hitherto remained unknown to all conchological writers. Its extreme length is rather more

than eight inches and a half; and its greatest breadth (from the ligamental to the basal margin,) five inches three-quarters.

Its form is a broad oblong-oval; obtuse at both extremities; the anterior of which is broadest and sinuated, and the posterior rounded, and so very short as to project only three quarters of an inch beyond the outer side of the cardinal teeth. The whole shell is remarkably flat but very thick in substance; and the umbones, which are unusually small have scarcely any convexity; the ligamental margin is dilated, winged, and forming in its dilation nearly two equal sides; the horny part of the ligament itself, (with the internal plate that supports it,) extends half way between the umbo and the extremity of the wing; the exterior colour of the epidermis, is dark brown, but the umbones (in this specimen) are decorticated for a considerable space around them. The sculpture of the anterior part of the shell is very peculiar; it consists of four series of short oblique grooves, or of indented wrinkles, three of which are arranged in a direction with the umbonial slope*, the other is transverse; the first of these series consists in parallel grooves which cross the wing obliquely from left to right. The next is a range of broad and sinuated indentations, wide apart and having the same inclination as the former; the third range occupies the umbonial slope, and is formed by narrow sulcated grooves, placed nearly in a horizontal direction, and diminishing in length as they approach the umbones. The fourth and last consists of several transverse grooves situated near the basal margin, and the whole presents an appearance as if the shell had been indented, in various directions, by some blunt instrument.

The inside is pearly, white, tinged with flesh-coloured purple, and stained (as is frequently the case, in fluviatile bivalves) with olivaceous yellow spots; in a perfect state of the shell the colours, probably, would be more brilliant.

The cardinal teeth are strong; deeply and irregularly striated and are obliquely transverse; in the right valve are two, and in the left valve, one; in each valve there is only one lateral tooth, a very unusual and discriminative character for the species; this

* See Zoologiæ Illust.

tooth is very thick, minutely crenated, and is double the length of the ligamental plate, which latter is much elevated, broad, and terminates abruptly in a sinus extending to the point of the wing: adjoining the cardinal teeth are four deep muscular impressions, one of which is very large, and two of the others very small; the anterior impressions are slight and present nothing peculiar.

Ob. Two odd valves of this unique shell came into the possession of Mr. G. Humfrey, A. L. S., many years ago; and were sold with part of this gentleman's collection last spring; the shell then passed into the hands of Mr. Mawe; Mr. H. was informed it came from the river Oronokoo; this I think a very probable locality, for it has all the characteristics of an American species; its massy substance and uncommon size seems, moreover, in unison with the force and rapidity of such a vast river. I am not well satisfied as to the exact form of the dilated process on the anterior side; as in both these valves the edges had been injured and repaired: in the perfect shell this part probably may be more dilated, and may terminate in a form somewhat different from that which I have described.

Warwick, 1824.

ART. III. *On Indistinctness of Vision, caused by the presence of False Light in Optical Instruments; and on its Remedies, by* C. R. Goring, M. D.

[Communicated by the Author.]

OPTICAL instruments in general have within the last century been brought to so high a degree of perfection, that it may almost be doubted if there remain any real improvement to be made in them; nevertheless, it has appeared to me, that in the humble department of their construction which provides against the admission of false light, there is still left some capability of a further advancement towards perfection, which may be effected with advantage, to the performance of astronomical refracting telescopes, Newtonian reflectors, and compound microscopes.—As it is necessary to understand the nature of an evil before we can cure it, as

well as to feel the utility of removing it, I shall here give a slight account of the indistinctness occasioned by *fog*, (as it is technically termed) to which I propose to apply a remedy. Thus, when we look through a telescope admitting false light at a printed bill, the plate of a clock, or other such object, especially if the day is clear, and the sun shines on it, we find, (however perfect the instrument may be in other respects,) the letters or figures do not appear nearly so black and sharp, as they will when viewed by the naked eye under the same angle, but rather of a brownish colour; in other words, the effect upon the eye, is similar to that of looking through a mist, or through glasses dimmed by moisture ; in short, what an ordinary observer would express by saying, the instrument *did not shew objects clear and distinct* *. Now, on examining the pencil of rays proceeding from the eye-piece of such a telescope, with a magnifier, it will (supposing no other source of indistinctness exists,) be found surrounded by a variety of foreign rays, forming different halvos about it, instead of appearing like a spangle on a piece of black cloth, which it will do, when all the false light is stifled, as in Gregorian and Cassagrain reflectors by their eye-hole, and in refractors with erecting eye-pieces, which have a stop between the two bottom glasses, producing the same effect, by suffering nothing but the true and genuine pencil of light, from the object glass or metal, to reach the retina. Indeed, in these instruments the quantity of spurious rays would be so great, as absolutely to preclude any thing like distinct vision, without the stops and eye-holes in question. In those to which I propose to apply an equivalent contrivance for extinguishing fog, though there may not be the same imperious necessity for its application ; still I think the advantage to be gained by the improvement, is not to be despised, but will rather be admitted to be highly useful and appropriate, as placing optical instruments one step nearer perfection than they would otherwise be, by producing the maximum of distinctness and clearness of vision, of which they can be rendered susceptible, consistently with their excellence in other respects.—

* All objects are of course equally affected by the fog, but it is more striking in those I have designated than in others.

Now, opticians have not been entirely insensible to the advantages, to be obtained by excluding all inefficient light;—being aware that no kind of blacking applied to the inside of an optical tube, is sufficient to effect that salutary purpose, they have had recourse to other means, though inadequate to the end in view.—Thus it is common in a refracting astronomical telescope, to meet with one stop and sometimes two, placed in the interval between the object and eye-glass; the apertures of the magnifiers, are likewise contracted on the same principle. But these stops are never in sufficient number, or sufficiently contracted, or placed in such situations as they should be to be efficient, at least it has never fallen to my lot to see any such. It seems to me, as if they were possessed of some superstitious dread of cutting off some of the light of the object glass by inserting stops; or perhaps have wished to shew their customers, that the apertures of their glasses were clear, it being a common trick to make a large object glass, and then to cut off the effect of the imperfect edges by a contrivance, such as has been mentioned, which ordinary purchasers are not aware of, and thus, suppose, the instrument to be much finer and better than it really is; at least it is not uncommon to meet with this species of fraud in the works of the continental artists, who are very fond of making larger object glasses than the English workmen. To enter into my subject, I shall here as succinctly as possible, describe the method which I have experimentally found to answer best for stifling fog in the astronomical refractor. It is a consideration which must obviously present itself, that if an eye-hole be placed at the end of a telescope, precisely of the size and precisely in the focus of the pencil of rays produced by any particular magnifier, that the end here proposed will be attained, as in the Gregorian and Cassagrain telescopes; it will moreover, confine the eye truly to the axis of the tube, and thus prevent us from seeing any of that colour in the image, which may always be perceived in the best instruments, when the eye is a little removed from its true position. Nevertheless, I have after sufficient trial rejected this method, as less expedient than another which I shall point out, on account of the difficulty of executing it

properly with high powers, as well as that it confines the field of view, and is disagreeable to the eye. It is evident, that with high powers the pencils of rays will be exceedingly small, therefore if the aperture of the eye-hole is too large it will be ineffectual, if too small it will obstruct light: it must therefore be executed to a very great nicety, which is not always to be expected; besides in a case of such delicacy, if the eye-piece be not screwed on to a particular mark on the body of the tube, or if any of the parts of which the magnifying apparatus is composed, be more or less screwed home than at the time of adjustment, it will be highly probable, that the eye-hole will be a little thrown out of its true situation, and thus do away with the sole object for which it was constructed. Thus it is, that what is perfect in theory, will not always answer in practice. As to using eye glasses with very small apertures for the same purpose, (as contradistinguished from eye-holes, placed at the ends of the cones of light, drawn to a point by the magnifiers,) it is a method which cannot be made to exclude false rays with any degree of precision, even though their diameters are so much reduced as greatly to contract the field of view. I shall now give an account of the plan I have selected as most eligible, and which I have applied to a thirty inch and eighteen inch refractor with complete success, as it seems to me. Fig. 1. Plate II. is a drawing of the section of a refractor, in which may be seen seven stops in the course of the tube and eye-piece, (exclusive of the field bar,) five of which are placed in such a manner, and of such apertures as to pinch the cone of rays proceeding from the object glass as tight as possible, without intercepting any. It will be obvious that no foreign rays, or any that are not parallel, will be able to find their way to the eye, nor can any light be reflected from the sides of the tube, so as to become visible. To execute this, use the following method; when the telescope is finished in the usual way, and before any stop is inserted, attach its lowest astronomical eye-piece to it, and find the true solar or sidereal focus of the object glass; when thus adjusted, measure carefully with a dynameter, the size of the pencil of rays proceeding from the eye-piece, and note it down. Having then

procured a plank of wood covered with paper of sufficient length, take the aperture of the object glass, and set it off at one end of the board, bisect it and draw a line at right angles to it, to the exact length of its focus: fix three strong needles into the three points of the focus and aperture of the object glass, and then stretch a fine thread over them, which will then represent the cone of light which forms the image; set off six or seven inches from the focal extremity, (an efficient stop cannot be placed nearer, without contracting the field of view,) and ascertain the distance between the threads at this point, which will give the diameter of the fifth stop. Then divide the remainder of the focal length into five equal parts, (whatever it may be,) and the distance of the threads will give the diameter of four more stops, 4, 3, 2, and 1, in the figure, all sufficiently correct for the purpose. The stops are then to be made and inserted into the tube in their proper places; it will not however be amiss to make No. 1, 2, 3, and 4, a little too large, and to confide the main business of stopping the false rays to No. 5*, which may be attached to the eye tube, and move along with it, in adjusting the focus of the magnifier. This will give the instrument the power of adjusting itself, to nearer objects on the earth, without losing any light from the effect of the stops, which otherwise must be adjusted to the shortest focus of the object glass, and supposed to act perfectly only with parallel rays. It will not be amiss to have a very small eye-hole, placed correctly in the focus of the object glass, which will give a great facility of adjusting the stops, as it will shew by merely looking through the instrument, if they are correctly placed or not. Lastly, having fixed these, apply the eye-piece, carrying the lowest mag-

* The way of regulating the aperture of this or any other stop to its situation in the course of the tube, will naturally be by pushing it up or down, till it strictly conforms itself to the size of the cone of rays at the point where it acts; when once it is settled for the lowest magnifier, so that the image of its aperture, and that of the object glass correctly correspond, and shew no difference in measurement by the dynameter, the business is effected for all the other powers, as they will always preserve the same relative proportion to each other, whatever may be the depth of the lens which is employed to form an image of them.

nifier as before, and again with the dynameter measure the size of the cone of rays at the eye; if it measures precisely as before, you may be quite confident you have cut off no true light*. I dare say it will be thought, there are already a very superabundant quantity of stops, but, I am sorry to say, that on examining the pencil with a magnifier, it will most probably be found, that some false light is still reflected from the eye tube, to cure which two more stops will be necessary, (Nos. 6, and 7;) both of these however, must be larger than No. 5, and No. 7 the largest of the two, or the field of view will be contracted in the low powers.—The higher the power, the nearer an efficient stop may be placed towards the focal extremity of the pencil, proceeding from the object glass; the lower it is, the farther the stop must recede, gradually of course, increasing in its aperture, (unless the length of the eye-tube is increased in proportion to the focus of the magnifier employed.) I have pitched upon six or seven inches, which is a distance for the main stop, that will suit all telescopes, and all magnifiers which are not more than one inch focus. Here it seems proper to observe, that when once the false light is duly excluded from a telescope, in the manner I have here recommended, the eye glasses may be used of any aperture, and thus the field of view may be had of any size, even with a single eye-glass, which in my opinion, when high magnifiers are used is a great convenience, as it enables us to keep a celestial object in sight more easily, though we should only see it distinctly in the axis of the telescope; moreover, should it not be thought worth while after all, to have the false light as perfectly excluded, as it possibly can be under all circumstances, the stops No. 1, 2, 3, and 4, may be rejected, and 5, 6, and 7 only executed; the consequence will only be, that some false light will be rendered sensible, when the eye is not confined to the centre of the eye-glass.

* I think an instrument from which all the false light is utterly excluded, does not appear quite so luminous as it did before, for false rays are as capable of affecting the retina, as true ones. [If we were to turn out all the disagreeable people out of a room full of company, there would of course be fewer individuals in it, but the society would, I think, be indubitably improved by the measure.]

I have given the full complement of stops to render the exclusion as complete as possible. As good a way as can be devised of illustrating the effect of the stops, is the following. It is known that a terrestrial telescope with an erecting eye-piece of four glasses, would be rendered nearly useless by withdrawing the little stop placed between the two glasses which erect the image, by the quantity of fog which would be let in, (supposing the instrument constructed in the usual way.) But in a telescope furnished with such stops as I have described, it is of no consequence whether the little stop is introduced or not, the performance is precisely the same in point of distinctness in both cases. In performing this experiment, it is necessary however, that the aperture of the little stop should be correctly and truly accommodated to the size of the pencil, which is formed by the glass, in the focus of which it is placed, so that it shall barely admit the image of the object glass, without cutting any of the side rays off, otherwise the experiment will not be fair.—Opticians are apt sometimes to make these apertures so small, as to intercept some of the light of the object glass as effectually as a cap over the end of it would; for the achromaticity of an erecting eye-piece depends very much upon the size of the little aperture in question. Thus where the diameter of an object glass bears a very large proportion to its focal length, it will be impossible to admit the whole of the light proceeding from it into the eye-tube, without at the same time destroying the achromatic property of the latter, by the necessity which would arise of opening this stop too wide to consist with it. In the experiment I have detailed, there is sufficient proof that the effect of my stops is equivalent, indeed more than equivalent, to that produced by the stop which is inserted in erecting eye-pieces for the purpose of procuring distinct vision. What then? is there any merit in having effected, by means of half a dozen stops, what may be done well enough by one? certainly not. But take away the erecting eye-tube, screw on an astronomical eye-piece, (either with or without a field bar,) and where I ask now, is that part of structure which is to do the work which was performed in the erecting compound magnifier, now removed, by the little stop of which I have said so much; (supposing the telescope constructed in the usual

manner, that is to say, without stops in the course of the focal distance of the object glass, or at least without effectual ones?) It cannot be asserted, that there is anything equivalent in the instrument in its present condition, to the former provision in it, for the valuable purpose of excluding false rays, though the expediency and utility of it in both cases, must be equally admitted or denied, and it is clear this can only be supplied in the astronomical telescope, by some such expedients as I have resorted to.

As I conceive no one can be hardy enough to assert that there is no use in excluding false rays from a telescope intended to be used at night, for viewing the heavens, it will be superfluous for me, to set about proving that we shall see a celestial object the better, if no light, either direct or reflected, reaches the eye, save that actually proceeding from it. If the light of the heavens in a star-light night, and that of the bodies which produce it are very faint, still there is the same ratio between their brightness, and the false light they produce, (though not so conspicuous perhaps) as there is in that of terrestrial objects. Indeed it is perfectly well known to astronomers, that in the darkest night, wearing a black hood over the eyes, greatly facilitates the vision of very faint and delicate objects, such as nebulæ, &c., from the sensibility and tranquillity induced by these means in the retina, rendering it susceptible of the slightest impressions. Surely the effect of foreign light reaching the eye directly, or through the medium of a telescope, must be equally pernicious. It is in viewing the class of objects here designated, that the utility of the stops I have described will be found; no one will expect that they can render a telescope better able to define or divide a star, because these properties depend upon the perfection of an entirely different part of its structure. I shall take my leave of the subject, by asserting that if any one should choose to maintain a contrary opinion from myself on the affair of excluding false rays, he must, to preserve consistency, assert that there is no use in the eye-hole of the Gregorian and Cassagrain telescope, (if used at night,) and should therefore in using these instruments, content himself with such a one as is applied to common spy glasses, just to keep his eye in the axis of his instrument.

I now proceed to the next part of my subject, which is the consideration of the Newtonian telescope. This admirable instrument, such as could only be expected from the genius of the immortal philosopher who invented it, has, (as a necessary consequence of its construction,) among its other valuable peculiarities, that of having less false light in it than any other kind of telescope.—The same striking effect, therefore, will not be manifest in excluding the trifling fog there is in it, as in another construction where it is more abundant. Nevertheless there is something to be done.—If we examine the pencil of rays proceeding from its eye-piece with the magnifier, it does not precisely represent the image of a spangle or a piece of black cloth, as it should do;—a good deal of foreign light may be seen, formed partly by the side of the tube behind the diagonal metal, and partly by such portion of the end next the large mirror, as the plain one can reflect along with the image, together perhaps with some reverberated by the little tube which carries the magnifiers.

In Fig. II. is represented the method I have taken to remedy these imperfections in a 77-inch focus, and seven-inch aperture Newtonian*. The alterations from the common construction, are

* On exhibiting these alterations to those celebrated artists, Messrs. Tulley of Islington, I learnt from them that they had lately made an arrangement of the same description, in a Newtonian telescope, made for Mr. Camfield of Northampton. These gentlemen (whose unrivalled pre-eminence in their profession, needs not my feeble testimony, or eulogium) fully admit that the exclusion of the false light, makes a great difference in the performance of the telescope, *in the day-time*, but do not seem to think any alteration is to be perceived in viewing celestial objects. I am aware, that I am paying myself an indifferent compliment in differing from such authorities. I have given my reasons for so doing, and cannot help still being of opinion, that it is scarcely possible to select any object in the heavens, and to view it without rays from a variety of others also finding their way into the telescope, and thus disturbing the singleness of vision, which would exist was there but one star or object in the heavens to emit light. I think, in particular, that the double ring of Saturn, and its belts and shadows are not perfectly seen, unless the telescope employed to view them, will show *black objects perfectly black*, and *white objects perfectly white*, and of course all the intermediate gradations of shade correctly; very few telescopes will shew the division between the rings of Saturn *quite black*, (as mine does,) owing to the false light so generally prevalent in them.

as follows: the tube which carries the magnifiers is seven inches long, instead of being only two inches or perhaps less, as is usual —the diagonal metal is likewise placed nearer than usual to the large one, so that the length of the telescope is reduced about five inches; this is of course necessary to render the pencil of rays reflected at right angles from the axis, long enough to act with the increased length of the eye-tube. (As the diameter of the spectrum of the great mirror increases as we recede from its focal extremity, more of the small plain one will in this case be called upon to act; it however will still do the work without any increase in its size; mine is $1\frac{3}{10}$ inch of circular diameter, yet its entire surface is not employed.) By this arrangement a sufficient length of eye-tube is obtained to insert the stops 5, 6, and 7, as in the refractor, No. 5, is the efficient stop as before and is $\frac{7}{20}$ inch in diameter. The extrusion of the aberrant light is complete as long as the eye is in the axis of the instrument.

It would of course be impossible to insert any stops similar to those, 1, 2, 3, and 4, in the refractor to render the effect more complete; nevertheless, I think, were it any object, a Newtonian would by the aid of the contrivance I have applied to mine, act sufficiently well with a skeleton tube only. It now remains for me to describe the new adjustment which the adoption of so long an eye-tube has compelled me to have recourse to, for it is evident that any want of centricity and parallelism in the lenses composing the eye-glass to the axis, which might be tolerated in a very short tube, will be perfectly insupportable when aggravated by a longer one: moreover, the stop No. 5, (which I suppose to be so adjusted as barely to suffer all the light of the great metal to clear it) will, if not truly concentrical with the cone of light on which it operates, evidently impede some, as in such a case is perceptible by looking through the small eye-hole recommended in adjusting the refractor, or by examining the extreme pencil after it has passed the eye-glass with a magnifier. It is evident, I think, that the adjustment of a Newtonian is complete, when the pencil of rays which is reflected from the small metal, truly perforates the axis of the eye-tube, and the centres of the lenses composing it; it mat-

ters not, I conceive, at what angle or in what direction the said cone of rays proceeding from the large metal be thrown by the diagonal one, provided these conditions are fulfilled, (supposing of course the position of the small metal to be the centre of the tube, so that it shall truly receive the whole of the light of the great one.) We may, therefore, either adjust the small metal to the eye-tube, or the eye-tube to the small metal, or we may do both, which latter will probably be the most expedient, and is the method I have preferred; I have effected it in a very simple manner, by having the tube made to fit loosely into another wider piece, which is screwed on in the usual way, to the side of the telescope:—the vacancy between them is filled up with wax, the inner tube is tight at the bottom of the external one, by the interposition of a small setting chamferred at the edge, but admits of a slight rotatory motion towards the eye-glass by heating the wax with the flame of a candle which is inserted into the external tube, and which unites them both together; time will be given to adjust it before the wax cools, when it will all set tight, and will not be liable to get out of order. Two small niches should be made, one in the shoulder of the screw of the external tube, and the other in that of the female screw to which it is applied, to be a guide that the two pieces may always be screwed home to a particular point: or it is very probable the adjustment may be spoiled, because it will be a chance if the eye-tube when fixed, is precisely at right angles to the side of the telescope. A variety of methods of effecting this adjustment will present themselves to the workman, instead of that which I have used, which though it answers very well, yet does not look very elegant or scientific. Thus, instead of the wax, three screws might be used, fixed into the external tube; or such a contrivance as is represented in Fig. II., by having counter screws to play against those by which the setting for the eye-piece is attached to the rackwork, on the side of the telescope, &c. Now it is not my intention to assert that this adjustment is absolutely indispensable, for I have not a doubt but that a superior workman might execute a long eye-tube, such as I have employed, so that nothing but the usual adjustments would be required; still I think that no Newtonian would be

injured by having such an apparatus to it as I have recommended, even though the eye-tube were only of the common length; it is certain it could do no harm at least. I think I can, moreover, with confidence assert that increasing the distance between the small metal and the eye-glass, for the purpose of applying stops, will not be found to make the least sensible difference in the performance of an instrument, as far as the figure of the small metal is concerned, provided it is of the standard goodness*; if but imperfect such an alteration will evidently try it more, and this will be shewn by examining a double star which will probably vary slightly in the distance at which the stars appear separated, (cæteris paribus,) according as the eye-glass approximates to, or recedes from, an imperfect diagonal. To conclude, as an Herschelian telescope is nothing but a Newtonian, used without the interposition of a small metal reflector, whatever has been said of the latter, will equally apply to it, and the same principle in the eye-tube and adjustment, will for the same reasons be equally adapted to both, though the manner of execution will be different; I have, however, made no experiments on this kind of telescope.

[The portion of this paper relating to Microscopes is reserved for our next Number.]

Art. IV. *The Characters of several New Shells, belonging to the Linnæan Volutæ, with a few Observations on the present State of Conchology.* By William Swainson, Esq., F.R. and L.S.

The study of conchology has now become so general, or, if I may be allowed the term, so *fashionable*, that the number of elementary works is truly surprising. The new systems of the French conchologists have been translated, explained, and advocated, in various publications; while the admirers of the Linnæan method,

* It is evident that if the diagonal metal were quite perfect, it could make no difference at what distance the eye-glass was placed from it; if decidedly imperfect, it is no less plain that the nearer the eye-glass is placed to it the better, because the less of the edges will be called into action, which will of course be the worst part.

have not been backward in expressing their warm attachment to the plan of the great Swedish naturalist. It is not my present intention to speculate upon the respective merits of these systems. In the study of no class of the animal kingdom have there been so few absolute facts discovered, whereon to build a truly natural system, as in that of the testaceous mollusca. In the history of those families which are known, anomalies have been discovered, which baffle explanation, and obstacles almost insurmountable, from the very nature and *habitat* of the animals, conspire to retard that rigid investigation of their economy; which must alone form the basis of their perfect arrangement.

But while so many writers have been engaged in forming systems and constructing genera, the elucidation of *species* has comparatively been neglected.

An extensive acquaintance with species is the first step to a knowledge of natural divisions. In every branch of natural history those who have seen and studied the fewest individuals, will be most apt to create new genera; "when they have seen more, they will discover the intermediate links which unite different genera; and thus be forced to join what they formerly separated *". I am fearful this has not sufficiently been considered by the authors and advocates of the French systems: it may be doubtful if their generic distinctions are not too much refined; but it is certain that a knowledge of the science is daily becoming more unattainable to all but professed naturalists.

While this revolution of classification and of genera is going on, our cabinets are crowded by innumerable species, some of which we know not how to name, while others (well known by the figures of the older writers) remain undescribed: new species are continually pouring in upon us to augment the number: and although the student may be perfect in the elements of his system, he knows not how to proceed, or where to turn, if he ventures on the investigation of species.

The volumes of Lamarck (His. Nat. des Animaux sans Vertebres) have indeed done much to remedy this evil. They contain

* Wildenow, Principles of Botany, P. 175, Sect. 163.

a considerable increase of new species, and a more perfect elucidation of many of the old ones; but, on the other hand, the same over-refinement which marks the characters of his genera, will be traced in the discrimination of his species; this is more particularly the case in his account of the genera Conus, Oliva, and Helix. Let me not, however, be misunderstood, as wishing to depreciate the merits of this great man. His general reputation could not be affected either by my praise or my censure. But obliged, as he has been, to employ the sight of another in finishing his latter volumes, *it would perhaps have been better for his own sake,* and that of the science, to which he has devoted his long life and great abilities, if they had never been published.

The importance of monographs, or complete histories of particular tribes, or families, in every branch of natural history is unquestionably very great; for their object is, not only to ascertain the limits of genera, and the affinities and analogies, which the individuals of such genera bear to others, but likewise to include the history of all the species thereunto belonging. To accomplish this, however, is in the power of a very few. Access must be had to the rich contents of foreign museums, and of costly libraries, to supply what may be deficient in minor collections; and it is from this cause that nearly all the monographs of extensive families have proceeded from naturalists in the charge of public museums, or in the possession of immense private collections. From the labours of these men science has received the greatest assistance. But, although few can enjoy the advantages such materials afford, considerable benefits will be derived from the labours of those, who frame a correct diagnosis of individual species; particularly when relative characters are subjoined, and comparisons made between others to which they bear a resemblance. When it is considered how many rare and unknown shells have lain for years in the cabinets of mere collectors, and how much greater is the number of those species more usually seen, but which are likewise unrecorded, the value of these isolated descriptions will be rightly understood. They are the indispensable materials for completing a general survey of the natural world, and constitute the ultimate object of

all systems; namely, such a knowledge of the individuals, as will enable the student to assign to each "a local habitation and a name."

I shall now proceed to describe several beautiful shells; mostly of uncommon rarity, and apparently unknown to modern writers: the four first belong to the genus *Voluta*, as it is now restricted; and the remainder to *Mitra*, a genus to which I have long paid much attention, with the ultimate hope of illustrating it by a distinct monograph.

VOLUTA. Lam. (Div. 1. Musicales.)

Voluta *chrysostoma.*

V. testâ ovatâ, albente, lineis angulatis maculisque castaneis ornatâ; anfractibus spinis brevibus, concavis coronatis; apice crasso, obtuso, lævi; aperturâ aureâ.

Shell ovate, whitish, with angulated chestnut lines and spots; whorls crowned by short concave spines; apex thick, obtuse, smooth; aperture golden.

Voluta chrysostoma. Sec. Exotic Conch. Fas. 5. ined.

Voluta luteostoma? testa obovata, angulata, lineis et venis fuscentibus in fundo albido undulata sub-perforata, anfractibus cinctis nodis conicis, apice obtuso, basi valde emarginata, columella plicata plicis quatuor solidis, fauce lutea. Chemnitz xi. p. 18. tab. 177. F. 1707-8.

DESCRIPTION.

The shell in its habit, approaches V. *vespertilio:* its total length is about two inches, of which the spire occupies not more than half an inch: its form is oval, and its surface without sculpture: the basal volution, and the two first whorls of the spire, are crowned by a row of short thin vaulted spines, rather acute, and resembling those on V. *diadema*, (Ex. Conch. Fas. 1.) The remaining three spiral whorls are perfectly smooth, the middle one being by much the largest, and the whole forming a thick and somewhat obtuse cone. The base is deeply emarginate, and the plaits on the columella, (which are four in number) are very thick. The ground colour of the specimens before me is nearly white, with broad

longitudinal shades of deep chestnut, broken into rows of angulated whitish spots of various sizes, and disposed in a longitudinal direction. The spire is white, with a few brown undulated lines on the lower whorls; the inner lip yellowish white, and the throat or *inner aperture golden yellow.*

This shell, as far as regards English collections is unique. It is now in the possession of Mr. Mawe, I believe, and in all probability may be found to inhabit the Indian Ocean.

The *Vol. luteostoma* of Chemnitz,(a shell passed over by Lamarck, and all systematic writers,) bears a strong resemblance to this species, but I have many doubts if it be really the same. In this genus, the form and sculpture of the terminal whorls of the spire, afford the most certain specific distinctions; now in the V. *luteostoma*, these terminal whorls are represented as graduating to an obtuse point, whereas in the shell above described, they are very thick and papillary. V. *luteostoma* is stated to be " *subperforata*," but this V. *chrysostoma* bears not the slightest indications of such a character, neither is the description, " anfractibus cinctis *nodis conicis*," applicable, if intended for the latter species. On the other hand, the two shells agree in their general form, habit, the golden colour of their apertures, and nearly the pattern of their markings. I have preferred however, for the present, to keep them distinct; because every conchologist must be sensible, more perplexity has been introduced into the science, by creating too few species than too many.

Voluta Gracilus.

V. testâ oblongo-fusiformi, lineis undulatis pictâ; spirâ producta plicatâ; labio exteriore subreflexi; columellâ 4 plicatâ.

Shell oblong-fusiform, with undulated lines; spire lengthened, plaited; outer lip sub-reflected; pillar 4 plaited.

Description.

This is a most elegant shell, belonging to the same group as that filled by V. *undulata* and its allies; from all of which it may at once be known by the great prolongation of its spire, which is nearly the length of its aperture. The whole shell does not exceed

two inches and a half in extreme length; the basal volution is smooth; but the three next whorls of the spire are plaited, and slightly nodulous; these plaits then disappear, and leave the terminal volutions quite smooth; the apex is obtuse, but not enlarged; the base of the shell is contracted, and the emarginate notch rather slight; the margin of the outer lip is somewhat reflected, and on the columella are *four* slender and nearly equal plaits. The colour is pale brown, elegantly marked by longitudinal, slender, waved, and angulated lines of a deep fulvous brown; at the top, bottom, and middle of the basal whorl, these lines are more thickened and deeper coloured, so as to form three transverse bands. Another specimen of this species was covered over with a reddish tinge, which nearly obscured its markings. The brown lines are also continued on the spire, but are fewer and more remote.

Ob. Two specimens of this elegant voluta were brought home by one of the South Sea trading vessels from the Bay of Island, they are now in the possession of Mr. Mawe.

Voluta Costata.

V. testâ ovato-oblongâ, costis sub-mucronatis, pallidâ, lineis fulvis interruptis fasciatâ; basi granosâ; spira mediocris apice lævi, obtuso; columellâ multiplicatâ, plicis tribus inferioribus maximis.

Shell ovate-oblong, with sub-mucronate ribs, pale, and banded with interrupted lines of fulvous; base granulated, spire moderate, the tip smooth and obtuse; pillar many plaited, the three inferior plaits largest.

The situation of this species, appears intermediate between the *Vol.* festiva and mitræformis of Lamarck, but its form cannot be compared to any other. It is little more than two inches and a quarter in extreme length; the spire is rather produced, and occupies one inch. With the exception of the terminal whorl at the apex of the spire, (which is perfectly smooth and obtuse,) the whole shell is marked by numerous, regular, convex ribs, about the same thickness as the breadth of the space which occurs between them; these ribs form a row of short obtuse spines, which crown the summit of

each volution; leaving between them and the suture an open channel; at the base of the shell are deep striæ, which cross the ribs, and produce a rough granulated surface. The ground of the shell is pale flesh colour, crossed on the ribs by bands of short slender fulvous lines; interspersed by a few orange spots: the aperture is also flesh-coloured, and the margin of the outer lip sharp and rather inflected. The upper part of the columella is crossed by numerous slender plaits, and at the lower part are three others much larger.

Described from a specimen in the possession of Mr. Mawe.

Ob. 1. The V. festiva of Lamarck, except from his description, is a species unknown to me. According to the writer, it differs from V. *costata* in being fusiform and ventricose, resembling in shape V. magellanica; it likewise appears destitute of the numerous small plaits on the columella, and of the obtuse coronations, formed by the summit of the ribs. These two shells with *V. nucleus* Lam. (which is Voluta harpa of Mawe's Introd.) V. *mitræformis*, and another undescribed species in my possession, constitute a new group in the genus; characterized by having the principal plaits on the pillar, situated at the base of the aperture.

Ob. 2. It may be necessary to observe, that although two shells already appear in the Linnæan classification under the name of *voluta costata*, neither of them in fact, belong to this genus as it now stands. One of these (the voluta nassa of Gmelin,) is a young shell of a species of *Nassa* Lam.; the other, (Vol. costata of Gmelin and Dillwyn,) is the Mitra Subulata of Lamarck.

MITRA. Lam. Cuv.

MITRA tessellata.

M. testâ ovatâ, lævi, striis transversis remotis et punctis, albente, lineis fulvis transversis et longitudinalibus cancellatâ, labii interioris basi fuscâ; labio exteriore lævi.

Shell ovate, smooth, with remote transverse punctured striæ; whitish, cancellated by transverse and longitudinal fulvous lines; inner lip brown at the base, outer lip smooth.

DESCRIPTION.

Habit of Mitra pertusa, but is much smaller in size, and less

ventricose; the spire also is shorter in proportion, but more thickened and obtuse; total length one inch three quarters; the whole shell is crossed by delicate remote striæ, which are minutely punctured; the aperture is rather longer than the spire, and together with the inner lip is pure white, the base of the latter is however stained by dark chestnut brown; the outer lip is rather inflected and is perfectly smooth; this latter character will at once distinguish this species from all its allies; the pillar has four plaits. The colour is uniform yellowish white, with slender fulvous, transverse lines following the indented striæ; these fulvous lines are crossed by others more broken and produce a singular resemblance to the mortar divisions of brick work; adjoining the suture is a row of small fulvous spots.

This shell I only know from a beautiful specimen in the possession of Mr. Mawe.

Mitra guttata.

M. testâ ovatâ, sub-fusiformi, lævi, striis transversis punctis, fulvâ maculis albis variâ; labio exteriore crenato; columellâ 5-plicatâ.

Shell ovate, sub-fusiform, smooth with transverse punctured striæ, fulvous variegated with irregular white spots; outer lip crenated, pillar 5-plaited.

Description.

Habit of the last; but is a much smaller shell, having the base more contracted, and the tip more acute; the striæ also are deeper and the punctures larger; it seldom exceeds an inch in length; the whole shell is brownish yellow, irregularly marked by white spots and blotches, these last spread over the spire, and form an irregular band across the middle of the body whorl; the aperture is white, the outer lip crenated, and the pillar has five plaits.

Ob. Two specimens of this species are in my own collection; but I am unacquainted with its locality.

Mitra Fusca.

M. testâ crassâ, ovatâ, lævi, fuscâ; spiræ contractæ suturâ subtilissimè crenatâ; labio exteriore crasso, gibbo, lævi; columella 4-plicatâ.

Shell thick, ovate, smooth, brown; spire contracted, suture minutely crenated; outer lip thick, gibbous, smooth; pillar 4-plaited.

DESCRIPTION.

Length one inch, habit of M. *crassata* Sw. the basal volution is thick and rather ventricose; the spire is short and abruptly slender, having the upper margin of the whorls projecting beyond the suture, and minutely but regularly crenated; the whorls are likewise crossed by a few remote striæ, the inner margin of the lip is thickened, and gibbous, except at the aperture, which is white. The whole shell is of a uniform fulvous brown; inhabits the Indian ocean, and is very rare.

MITRA ACUMINATA.

M. testâ crassâ, lævi, albente; spirâ contractâ, attenuatâ acuminatâ, aperturâ longiore; labii exterioris crassi margine inflexo, lævis; columellâ 4-plicata. Shell thick, smooth, whitish; spire contracted, attenuated, acute, longer than the aperture; outer lip thick, the margin inflexed and smooth; pillar 4-plaited.

On a cursory glance this shell (if in an imperfect state) might easily be mistaken for the last; when perfect, it is about an inch and a half long (the spire occupying considerably more than half this length) and is faintly striated. The spire when uninjured is long, abruptly contracted, and terminates in an acute point. This part is so delicate, that in two specimens out of three which have come under my notice, it was wanting. The whorls are crossed by delicate indented striæ, but are not in the least convex; this character gives the suture an appearance of being channelled; the outer lip is somewhat inflexed, the margin smooth, thick and slightly gibbous within the rim; the base is obtuse, and the whole shell white, covered with either a yellow or reddish brown epidermis.

Inhabits the Mauritius.—A small though beautifully perfect specimen is in the possession of Mr. Mawe.

MITRA CARINATA.

M. testâ gracili, fusiformi, fuscâ, anfractibus medio carinatis, juxta suturam striatis, columellâ, 4-plicatâ.

Shell slender, fusiform, brown, whorls carinated in the middle, and striated transversely near the suture; pillar 4-plaited.

Description.

A remarkably slender fusiform shell, about an inch long; the spire being of equal length with the aperture; the shoulder of the basal volution, and the middle of the spiral whorls are crossed by a carinated ridge; between which and the suture, are two or three elevated transverse striæ; the rest of the shell is quite smooth; the aperture is white, and smooth within; the inner lip marginated, and the pillar 4-plaited. It is covered by a uniform brown epidermis, beneath which the colour is yellowish; base deeply emarginate, and slightly recurved.

Inhabits Sierra Leone, from whence it was received by Mr. Mawe.

It is a species at once distinguished by its crenated whorls, and should be placed in the same division as *M. vulpecula and melongena.*

Mitra Strigata.

M. testâ lævi, castaneâ, strigis longitudinalibus obsoletis, albentibus ornatâ; aperturâ spirâ breviore, alba; columellâ 4-plicatâ.

Shell smooth, chestnut, with obsolete longitudinal whitish stripes; aperture white, shorter than the spire; pillar 4-plaited.

Habit of M. *carbonaria*, Sw. and M. *melaniana*, Lam: the specimen before us measures two inches in length, the spire occupies an inch and one-tenth and is rather thick. The top of each whorl where it joins the suture, is turned and prominent, every part of the shell is destitute of sculpture and very smooth; the base is contracted and the pillar has four teeth, with the indication of a fifth. The colour is a rich glossy chestnut, striped at unequal distances, with paler, narrow, longitudinal stripes, which form dots of pure white adjoining the suture; the aperture and inner lip are also white.

The only specimen of this beautiful mitre with which I am acquainted, is in the possession of Mr. Mawe.

Mitra Bicolor.

M. testâ lævi, fusiformi, albâ, faciâ fuscâ latâ cinctâ, spirâ anfractusque vasalis parte superiore striis cancellatis punctis insculptis; striis basalibus simplicibus.

Shell smooth fusiform, white with a brown band; spire an

upper part of the body whorl with cancellated punctured striæ; base with simple striæ.

DESCRIPTION.

Shell about three quarters of an inch long, in shape, habit, and even in colour, resembling M. *casta* (Zool. Ill. pl. 48.) but the brown band, (which in that shell is merely formed by an external epidermis,) in this is internal, and delicately waved with capillary longitudinal lines of whitish; the longitudinal striæ are clouded and simple, but the transverse striæ are more remote, and deeply punctured; those in the middle of the body whorl, and of the base are likewise simple; the plaits on the columella are four, and very prominent; the base of the pillar is tipt with brown.

Ob. This shell, together with M. *casta, olivaria, dactylus* and *olivæformis*, constitute a particular group, distinguished by the plaits of the pillar extending far beyond the aperture.

Inhabits the South Seas? mus. nost.

ART. V.—*Account of the Earthquake in Chili, in November, 1822, from Observations made by several Englishmen residing in that Country.*

[Communicated by F. PLACE, Esq.]

CHILI is a long narrow country, lying between the mountains of the Andes on the east, and the Pacific Ocean on the west. It extends from 20° 20′ to 43° 50′ south latitude, and from 68° 50′ to 74° 20′ west longitude from Greenwich, its length being about 1350 miles, and its average breadth about 130 miles.

While under the dominion of Spain, Chili was visited by very few Europeans. Its great fertility, its abundance of metals and minerals, its agreeable and healthy climate, have, since it has been declared independent, induced a considerable number of Englishmen, and a few other foreigners, to become residents, and the number is continually increasing.

The country rises gradually but irregularly from the sea coast to the mountains; it is exceedingly diversified, but the principal

feature is its formation into valleys, surrounded by hills, many of them rising to a considerable elevation.

The whole country may be divided into two regions or climates, the one humid, the other dry, separated from each other by the river Maule, which in 35° 10′, falls into the Pacific Ocean.

South of the river Maule the climate is variable; rain falls at intervals during the whole year, and timber trees are in abundance. North of the river Maule the rains are periodical, and fall only during a particular time of the year. At Valparaiso, the principal sea-port of Chili, and for about forty miles to the northward, the rainy season commences in May and terminates in September. Further to the northward, the rainy season is of shorter duration, diminishing gradually, until at the northern extremity of the country, it totally ceases. To the southward of the Maule the time in which rain falls gradually increases, and, at the southern extremity of the country, there are but few intervals of dry weather.

Chili is never free from earthquakes; scarcely a week ever passes without one or more being felt, in some part of the country, but as the shocks seldom do any damage, the inhabitants pay but little regard to them.

It is now nearly a hundred years since the former great earthquake, and a persuasion seems to have prevailed among the people that no very considerable earthquake would happen oftener than once in two hundred years. Partial earthquakes, doing much damage, have always happened at intervals of a few years. The town of Coquimbo was nearly destroyed by an earthquake in 1820. The shock, was local, and produced no alarm in other parts of the country.

On the fourth of November, 1822, the town of Copiapo, in S. lat. 27° 10′, was visited by a severe shock, which damaged many houses; this was followed, the next day, by a much more violent earthquake, which nearly destroyed the town, and did considerable injury to the town of Coquimbo, in S. lat. 29° 50′.

The great earthquake on the night of the 19th of November, 1822, was felt over the whole surface of the country, from the mountains to the sea, and from one extremity to the other. Its force seems to

have diminished in a pretty exact proportion to its distance from Valparaiso.

Its effects are thus described by an Englishman, residing at Concon, near the mouth of the river named in the maps "Rio Quillota." Concon is about fifteen miles N.N.E. of Valparaiso, as the crow flies.

"At half-past ten, on the night of the 19th November, I felt the first oscillation. I was writing at the time; starting from my chair, I paused for an instant, expecting the shock would subside, as others had done; but the falling of glasses from the sideboard, the cracking of the timbers, and the rattling of the tiles from the roof, fully apprized the whole family of their danger, and all ran out of the house. The house was violently agitated, and was falling to pieces, but freed from the apprehension of being buried in the ruins, my attention was forcibly drawn to the phenomena, which I endeavoured to observe as accurately as possible. Scarcely, however, was this resolution taken, and before the first shock had entirely subsided, a second and much more violent one succeeded; this was accompanied by noise, which appeared to be deep seated in the earth, perpendicularly to the spot on which we stood. The duration of this shock was about two minutes; it was succeeded by a third, also accompanied by noise, less loud than that which accompanied the preceding shock. The shock was less violent than either of the two former shocks, and of less duration. These shocks occupied about five minutes of time. Shocks, at intervals, of four and five minutes, continued for nearly an hour, after which, they became less frequent during the remainder of the night, and of very different intensities, some being rather severe, and others hardly perceptible. The three principal shocks may be said to constitute the earthquake.

"At the commencement of the earthquake, the atmosphere was, as is usual at this time of the year in this country, quite free from clouds, the moon and stars shone with splendour; there was no atmospheric indication of change of any sort, either before or after the earthquake. Some persons say they saw an unusual light in the horizon to the southward, but I, who was expecting some

change, and was prepared to observe any that might have occurred, saw none whatever.

"During the earthquake the ground rose and fell with great violence, and with almost inconceiveable rapidity. There was certainly no undulatory motion, though many unobserving and unreflecting persons suppose this to have been the case. I had a strong suspicion at the time, since confirmed by observation of its effects, that there was a powerful horizontal motion, but as I could not perceive it as coming from any particular point, I concluded at the time that I was mistaken. The circumstances which make me now conclude there was a horizontal motion, are observations I have since made in many places, in which walls, and even houses, have been partially twisted round, and from the fissures round the roots of the largest trees. At Quintero, ten miles to the northward of Concon, are several large palm-trees; three of these standing so as to form an equilateral triangle, lashed one another like willow rods, and beat or shook off many of their branches. The motion of these trees seems to have been horizontal and circular, since each of them cleared a space in the ground round its stem, several inches wide, and this was the case also with other large trees in different places.

"The sensation we experienced during the earthquake, was probably the same we should have felt had we been conscious that a mine had been sprung beneath us, and was about to blow us all into the air.

"On examination next morning, at daylight, I found the earth full of fissures, some of them very small, while others were from two to three feet wide. In many places sand had been forced up, and had formed small hillocks. In the most recently formed alluvial soil near the river, water and sand had been forced up together, there being many large truncated cones of clean washed sand, each of which had a hollow in the centre, like the crater of a volcano. The same phenomenon was observed in several places; in other places, large quantities of soft mud had been forced up, and spread itself over the surface of the land.

"The surface of the country has been raised all along the coast, as far as my information extends. It seems to have been raised

highest at the distance of from two to three miles from the shore, diminishing both ways. The rise on the coast is from two to four feet; at the distance of a mile inland, the rise must have been from five to six or seven feet; for in the cut for the tail water course of a mill, at the distance of about a mile from the sea, a fall of fourteen inches has been gained in little more than a hundred yards.

"At Valparaiso, near the mouth of the Concon, and along the coast northward to Quintero, rocks have appeared in many places, where none before were visible. The high-water mark along shore is about three feet above the place the tide now reaches, and a vessel, which had been wrecked on this coast, and which could only be approached at low water in a boat, is now accessible on dry land at half tide*.

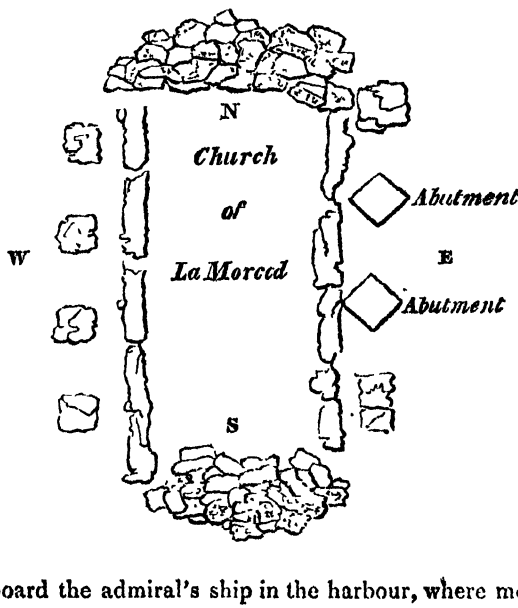
"At Valparaiso, not a single house escaped being damaged; it is somewhat remarkable, however, that although the ground was raised bodily, and considerably, those houses whose foundations were on the rocks, were less damaged than those built on the alluvial soil. All the houses at Valparaiso are built of adobes (sun-dried bricks), cemented with clay. These were thrown into heaps of rubbish, or torn and rent in all directions. The town had the appearance of having suffered a heavy and long continued bombardment. Upwards of three hundred persons were buried in the ruins. Had the earthquake happened two hours later, very few of the inhabitants would have escaped.

"After the earthquake, the inhabitants encamped upon the hills above the scene of desolation, in the best way they could; this was thought less of a hardship than it would have been thought in most other countries, from the fine warm weather, the certainty as was concluded of dry weather, and the small quantity of dew which, at this season of the year, falls in Chili. To these hills goods of all sorts, furniture, and every thing else, were brought, and laid in the open air. The damage done to this thriving town will not be repaired in many years.

"The church of La Merced presented a striking instance of the

* There is very little variation of tides on this coast, the sea never rises more than four feet at the full of the moon.

violence of the earthquake; the tower, sixty feet high, which served as a belfry, was levelled to the earth. Its solid walls of burnt bricks, well laid in mortar, were shivered in pieces; the two side walls, full of rents, were still standing, supporting part of the shattered roof, but the two end walls were entirely demolished. On each side of the church were four massive abutments, six feet square, of good brick work; those on the western side were thrown down, and broken to pieces, as were two on the eastern side; the other two were twisted off from the wall, in a north-easterly direction, and left standing.



"On board the admiral's ship in the harbour, where more secure than ashore, the effects of the earthquake, so far as the situation permitted, observations were made with great accuracy. Here three distinct shocks were felt, the second was observed to be by far the strongest, and its duration, as had been noticed at Concon, is stated at two minutes. The effect upon the ship was the same as would have been produced had she suddenly sunk down upon a rock. It appeared as if her bottom had been struck with prodigious force;

the vessel vibrated in an extraordinary manner, her timbers cracked, and she appeared strained throughout.

"At Santiago, the capital, at ninety miles distance from the sea, and about twenty miles from the mountains of the Andes, the earthquake was less severe; no houses were thrown down, although many, as well as the churches, were much damaged, but no lives were lost. Here, however, as in other places, the inhabitants removed from the town, and camped out in the open air. The effect of the earthquake at Aconcagua, about fifty miles N.N.W. of Santiago, was much the same as at Santiago. Millipilla, sixty miles S.E. of Valparaiso, suffered less than either Santiago or Aconcagua; but, at Casa Blanca, not a single house or wall of any kind was left standing. At Mapel, the shocks were very severe, great part of the village was destroyed, and a pool of water was formed in the market-place. Quillota also suffered to a considerable extent, many houses were destroyed, and all were more or less damaged. At Valdivia, in 39° 50′ S. lat., one shock only was felt; it is described as having been "pretty sharp," but it did no damage. At the moment the shock was felt, two volcanoes in the neighbourhood burst out suddenly with great noise, illuminated the heavens and the surrounding country for a few seconds, and then as suddenly subsided into their usual quiescent state.

"Although no atmospheric changes appeared at the time of the earthquake, there can be no doubt that very considerable changes took place. The weather, after the earthquake, continued as usual; but on the evening of the twenty-seventh of November, just eight days after the earthquake, the country, for a great extent, was visited by a tremendous storm of rain, accompanied with heavy gusts of wind; the rain continued all night, producing terror and dismay among the people. Every thing saved from the earthquake, was exposed in the open air, or under such temporary coverings as could be constructed with the few materials time and circumstances permitted. Few of the tents, under which part of the people lived, were water-proof. Many were living in enclosures called ramadas, made of dried boughs and bushes, open to the heavens, and many had no other fence than could be formed of

their furniture or other effects. Rain towards the close of the month of November had been expected by no one, and no preparation to defend either persons or property from its effects, had been made. Rain had never before fallen in the country, even at a small distance north of the river Maule, in the month of November. The consequences anticipated from the rain, which, from appearances, was likely to continue, were of the most distressing nature. The total destruction of the houses which had been injured, as well as that of the goods, merchandize, and furniture, which had been collected, and of the growing crops, was anticipated by all. Its immediate effects, had it continued, would have been intermitting and malignant fevers. These apprehensions caused the people to pass a night of indescribable agony. The rain, however, ceased suddenly towards morning, and the weather became settled as usual.

"The greatest force of the earthquake appears to have been felt at the distance of about fifteen miles N.E. of Valparaiso; the whole country, from the foot of the Andes to far out at sea, has been raised; the rise has, however, been very unequal.

"As the earthquake was felt at Copiapo in the north, and at Valdivia in the south, its extent, from north to south, exceeded nine hundred miles. Where the shocks were most severe, the earth has been raised the highest, and its not subsiding again to its former level has probably been occasioned by the innumerable fissures and multitude of small cracks caused by the repeated explosions, by which the density of the whole mass has been somewhat altered.

"Many persons to the northward of Valparaiso thought the direction of the shocks was from the south-west, while those to the southward thought they came from the north-west. If the principal force, as it appears to have been, was exerted within a circle of about fifty miles diameter, the centre of which was a little to the N.E. of Valparaiso, the direction of the shocks might have been, as those at a distance, to the north and south of that space, have described them. Most persons who live near the coast, suppose the shocks to have come from seaward, either to the northward or

southward, as had been mentioned, while those who resided within the circle described, conclude they were produced by explosions, perpendicular to the earth's surface. It does not appear that the earthquake extended into the mountains of the Andes; no change whatever was observed in any of these mountains, except as has been related near Valdivia, and here the volcanic ridge is nearer to the sea and less elevated than in any other part of Chili. The surface over which, or rather under which, the earthquake extended ashore, cannot be less than 100,000 square miles.

"During the earthquake the sea, for a considerable distance along the coast, receded and returned several times. At Quintero, the fishermen who live upon the beach, fled in terror to the sand-hills. At Valparaiso, a man-of-war's boat, going ashore, landed at the door of the Custom-house, which is twelve feet above the usual high-water mark. Neither the recussion, nor the retrocession of the sea, were as violent as might have been expected.

"Up to the end of September, 1823, the date of the last accounts, earthquakes continued to be felt; forty-eight hours seldom passed without a shock, and sometimes two or three were felt during twenty-four hours."

Art. VI. *On Evaporation. By* J. Frederic Daniell, Esq., F.R.S., M.R.I., *&c.*

[Communicated by the Author.]

The subject of evaporation has occupied, at various times, much of the attention of natural philosophers, and many accurate and interesting observations have been recorded of the formation and diffusion of elastic fluids, from various kinds of liquids. The circumstances, especially, attending the rise and precipitation of aqueous steam in the atmosphere, are acknowledged to be important in the highest degree, as upon their silent influence depends the adjustment of those important meteorological phenomena, with which is connected the welfare of all the organized creation. The labours of De Luc, De Saussure, and particularly of Mr. Dalton, have thrown considerable light upon this never-ceasing process; but

something appears to be still wanting to complete the investigation, and to follow up the results to their ultimate consequences. The following observations, however inadequate to fulfil this desirable purpose, may possibly attract some attention to the subject, and may be the means of indicating the points which most require elucidation.

It is a well-known fact that water, under all circumstances, is endued with the power of emitting vapour, of an elastic force proportioned to its temperature. It is also well understood, that the gaseous atmosphere of the earth, in some degree, opposes the diffusion, and retards the formation of this vapour; not, as Mr. Dalton has shewn, by its weight or pressure, but by its *vis inertiæ*. What is the amount of this opposition, and by what progression it is connected with the varying circumstances of density and elasticity, have never yet been experimentally explained.

It may facilitate the comprehension of the subject, to distinguish three cases with regard to the evaporating fluid: the first, when its temperature is such as to give rise to vapour equivalent in elasticity to the gaseous medium, and when it is said to boil; the second, when the temperature is above that of the surrounding air, but below the boiling point; and the third, when the temperature is below that of the atmosphere.

With regard to the first, all the phenomena have been accurately appreciated. The quantity evaporated from any surface, under any given pressure, is governed, in some measure, by the intensity of the source of heat, and is in no way affected by the motions of the aërial fluid. The elasticity of the vapour is exactly equivalent to that of the air, which yields *en masse* to its lightest impulse. When disengaged, it is immediately precipitated in the form of cloud, giving out its latent caloric to the ambient medium; and under that form is again exposed to the process of evaporation, according to the laws of the third division of the process. All the phenomena attending the process of boiling, have been ably investigated by Gay-Lussac, Dalton, Ure, and Arch-deacon Wollaston; but, as they have but little connexion with the atmospheric relations, which are the particular

object of the present paper, I shall proceed to the second case of evaporation.

When the evaporating fluid is of a higher temperature than the surrounding air, but not so high as to emit vapour of equal elasticity to it, the exhalation is proportionate to the difference of temperature. The gaseous fluid, in contact with the surface, becomes lighter by the abstraction of portions of the excess of heat, and, rising up, carries with it, in its ascent, the entangled steam. This, as in the former case, is precipitated, and, in the form of cloud, exposed to the third species of evaporation. This process is not only proportioned to the difference of temperature, and the elasticity of the vapour, but is also governed by the motion of the air. A current or wind tends to keep up that inequality of heat upon which it depends, and prevents that equalization which would gradually take place in a stagnant air. Such is the evaporation which often takes place in this climate, in Autumn, from rivers, lakes, and sea, and which is indicated by the fogs and mists which hang over their surfaces.

It is, however, the third modification of circumstances, which is the most interesting in the point of view which I have suggested, and from which I have merely distinguished the preceding, to free the subject from ambiguity. When the temperature of water is below that of the atmosphere, it still exhales steam from its surface; but, in this case, the vapour, neither having the force necessary to displace the gaseous fluid, nor heat enough to cause a circulation, which would raise it in its course, is obliged to filter its way slowly through its interstices; and the nature of the resistance it meets with in this course is the first object of investigation.

The force of vapour, at different temperatures, has been determined with great accuracy, and the amount of evaporation has been shewn to be *cæteris paribus*, always in direct proportion to this force. The quantity is also known to depend upon the atmospheric pressure, but I know of no experiments which establish the exact relation between the two powers. I attempted to elucidate the point as follows:—

By enclosing in a glass receiver, upon the plate of an air-pump, a vessel with sulphuric acid, and another with water, and by properly adjusting the surfaces of the two, it is easy to maintain, in the included atmosphere of permanently-elastic fluid, an atmosphere of vapour of any required force; or, in the usual mode of expressing the same fact, the air may be kept at any required degree of dryness. The density of the air, in such an arrangement, may, of course, be varied and measured at pleasure. Now there are three methods of estimating the progress of evaporation in such an atmosphere: the first, and most direct, is to weigh the loss sustained by the water in a given time; the second, to measure, by a thermometer, the depression of temperature of an evaporating surface; and the third, to ascertain the dew point, by means of the hygrometer.

Experiment 1.

The receiver, which I made use of, was of large capacity, and fitted with a hygrometer. I placed under it a flat glass dish, of 7½ inches diameter, the bottom of which I covered with strong sulphuric acid. The glass bell but just passed over it, so that the base of the included column of air rested everywhere upon the acid. In the centre of the dish, was a stand with glass feet, which supported a light glass vessel of 2·7 inches diameter, and 1·3 inches depth. Water to the height of an inch was poured into the latter, the surface of which stood just three inches above that of the acid. A very delicate thermometer rested in the water, upon the bottom of the glass, and another was suspended in the air. It may be necessary to observe, that the sides of the vessel were perpendicular to its bottom, which was perfectly flat. The height of the barometer was 29·6, and the temperature of the water 56°. In twenty minutes from the beginning of the experiment, the hygrometer was examined, and no deposition of moisture was obtained at 26°.

This being the greatest degree of cold which could be conveniently produced by the affusion of ether, the experiment was repeated, with a contrivance which admitted of the application of a

mixture of pounded ice and muriate of lime, to the exterior ball of the hygrometer. In this manner the interior ball was cooled to 0°; without the appearance of any dew. The temperature of the water and air were, in this instance, 58°, and the pressure of the atmosphere 30·5.

From this experiment it appears, that in the arrangement above described, the surface of water was not adequate to maintain an atmosphere of the small elasticity of ·068 inch; in other words, the degree of moisture in the interior of the receiver could not have exceeded 129, the point of saturation being reckoned 1000. How much less it was than this, or whether steam of any less degree of elasticity existed, the experiment, of course, did not determine. We may reckon, however, without any danger of error in our reasoning, that the sulphuric acid, under these circumstances, maintained the air in a state of almost perfect dryness.

Experiment 2.

The same trial was made with atmospheres variously rarefied, by means of the pump. No deposition of moisture was, in any case, perceived with the utmost depression of temperature, which it was possible to produce; and the state of dryness was as great, in the most highly attenuated air as it was in the most dense. In the higher degrees of rarefaction, the water however became frozen.

Experiment 3.

The water, which had been previously exposed to the vacuum of the pump to free it from any air in solution, was weighed in a very sensible balance, before it was exposed to the action of the sulphuric acid under the receiver. Its temperature was 45°, and the height of the barometer 30·4. In half an hour's time, it was again weighed, and the loss by evaporation was found to be 1·24 grains. It was replaced, and the air was rarefied till the gauge of the pump stood at 15·2; in the same interval of time it was re-weighed, and the loss was 2·72, but its temperature was reduced to 43°. The loss from evaporation, in equal intervals, with a pressure constantly diminishing one-half was found to be as follows:—

Pressure	Temperature. Beginning	End	Loss Grains
30·4	45	45	1·24
15·2	45	43	2·87
7·6	45	43	5·49
3·8	45	43	8·80
1·9	45	41	14·80
·95	44	37	24·16
·47	45	31	39·40

When the exhaustion was pushed to the utmost, the gauge stood at 0·07, and the evaporation in the half hour was 87·22 grains. During this last experiment, the water was frozen in about eight minutes, while the thermometer under the ice denoted a temperature of 37.

Now, before we infer from these experiments the state of evaporation, from different degrees of atmospheric pressure, it is necessary to apply to the results a correction for the variation of temperature which took place during their progress. The quantity of evaporation having been determined to be in exact proportion to the elasticity of the vapour, we must estimate the latter from the mean of the temperatures before and after the experiments, and calculate the amount for any fixed temperature accordingly. This will, doubtless, give us a near approximation, although, from the last experiment, we perceive that the method of estimating the temperature of the surface water cannot be absolutely correct. The following table presents us with the former results so corrected for the temperature of 45°:

Pressure.	Grains
30·4	1·24
15·2	2·97
7·6	5·68
3·8	9·12
1·9	15·92
·95	29·33
·47	50·74
·07	112·32

Notwithstanding the slight irregularity of the above series, we can, I think, run no risk in drawing from it the conclusion, that the amount of evaporation is *cæteris paribus* in exact inverse proportion to the elasticity of the incumbent air; and that De Saussure was misled by his hygrometer, when he inferred from its indications, that a diminution of one-third the density doubled the rate.

Before we proceed, it is necessary to say a few words upon the apparent discrepancy between the results of Mr. Dalton's experiments and mine, as to the amount of evaporation, at the full pressure of the atmosphere. He found, upon the supposition of no previous vapour existing in the air, that the full evaporating force of water, of the temperature of 45°. would be 1·26 grains per minute, from a vessel of six inches in diameter. This amount reduced in proportion to the squares of the diameters of the two vessels, would give 7·65 grains in half an hour, from the glass of 2·7 inches diameter, which I employed. It must, however, be recollected, that Mr. Dalton's calculations were founded upon experiments made at a temperature very considerably above that of the surrounding medium, and that consequently a current must have been established in the latter which greatly accelerated the progress. It is true, that he afterwards subjected his calculations to the test of experience, at common atmospheric temperatures; but then he expressly states, that "when any experiment, designed as a test of the theory, was made, a quantity of water was put into one of them (vessels), the whole was weighed to a grain; then *it was placed in an open window, or other exposed situation*, for ten or fifteen minutes, and again weighed, to ascertain the loss by evaporation." In this way he ascertained, that with the same evaporating force, a strong wind would double the effect. The difference, however, even after these considerations, is still very striking; but, from several repetitions of the experiment, I have no doubt of its exactness.

Experiment 4.

The arrangement described in the last experiment, having been found adequate to maintain in the receiver a state approaching to

that of complete dryness, I had no opportunity of judging whether the elasticity of the vapour, as it rose from the surface of the water, varied in any degree with the pressure of the air, or whether any part of the increase of evaporation were dependant upon such variation. To determine this point, I placed the sulphuric acid in a glass, of the diameter of 2·8 inches, so that its surface was very little more than equal to that of the water. The vessels were placed, side by side, upon the plate of the air-pump, and covered with the receiver. The temperature of the water and air was 52°, and the height of the barometer 29·8. The following table shews the dew point, which was obtained, at intervals of half an hour, at different degrees of atmospheric pressure:—

Barom	Temp. of Water and Air	Dew Point
29 8	52	36
14.9	53	37
7·45	52	35
3·72	53	36
1·86	52	34
·93	52	36
·15	52	36

The differences of these results are so extremely small, and are moreover so little connected with the variations of density, that there can be no difficulty in regarding them as errors of observation, and we may conclude, that the elasticity of vapour, given off by water of the same temperature, is not influenced by differences of atmospheric pressure. The equal surfaces of sulphuric acid and water here made use of, maintained, at the temperature of 52°, a degree of saturation equal to 570. I repeated the experiment, at the temperature of 61°, and the following are the results:—

Barom.	Temp. of Water and Air	Dew Point
29·6	61	48
14·8	61	49
7·4	60	48
3·7	61	50
1·85	61	48
·92	60	48
·15	61	48

Under these circumstances, the amount of saturation was 661; an increase evidently dependant upon the force of the vapour, but not in exact proportion to its augmentation.

Experiment 5.

Being now desirous of ascertaining in what degree the temperature of an evaporating surface would be influenced by differences in the density of the air, I made the following disposition of the apparatus:—To a brass wire, sliding through a collar of leathers, in a ground brass plate, I attached a very delicate mercurial thermometer; this was fixed, air-tight, upon the top of a large glass receiver, which covered a surface of sulphuric acid of nearly equal dimensions with its base. Upon a tripod of glass, standing in the acid, was placed a vessel containing a little water, into which the thermometer could be dipped and withdrawn by means of the sliding wire. The bulb of the thermometer was covered with filtering-paper. At the commencement of the experiment, the barometer was at 30·2 inches, and the temperature of the air 50°. Upon withdrawing the thermometer from the water, it began to fall very rapidly, and in a few minutes reached its maximum of depression. The following table presents the results of the experiment, for different degrees of the air's density; the intervals were each of twenty minutes:—

Barom.	Temp. of Air	Temp. of wet Ther.	Difference
30·2	50	41	9
15·1	49	37	12
7·5	49	34	15
3·7	49·5	31·5	18
1·8	49·5	28·5	21
·9	49	24·5	24·5
·4	49	23	26

Here, in an atmosphere which a former experiment has proved to be in a state of almost perfect dryness, we find that, at the full atmospheric pressure, the wet surface of the thermometer was reduced 9°. It is worthy of remark, also, how small a quantity of water is required to produce this effect. It has been previously

shewn, that a surface of 2·7 inches diameter, only lost 1·24 grains in half an hour. This would have been 1·41 grains at the temperature of 49° The surface of the wet thermometer could not have exceeded $\frac{1}{50}$th of that of the evaporating vessel, and the maximum effect was produced in ten minutes, or $\frac{1}{3}$ of the time, so that the weight of water evaporated in this case was not more than (·0094 grains) one-hundreth of a grain. It will be seen, that the depression increased with the rarefaction of the air, but in the proportion only of the terms of an arithmetical progression to those of a geometrical. The increase is attributable, not to the augmented quantity of the evaporation, but to the decreased heating power of the atmosphere. MM. Du Long and Petit, in their experiments upon the cooling power of air, determined it to be nearly as the square root of the elasticity; but whether the heat which it is capable of communicating to a cold body, follow the same progression, the experiments above detailed are not sufficient to determine with precision. We may, however, certainly conclude from them, that the temperature of an evaporating surface is not affected by the mere quantity of evaporation.

It is right to remark that, in the last experiment, care was always taken to station the evaporating thermometer in the same place in the receiver, for I found that, when the air was highly rarefied, a greater degree of cold could be produced by approximating the wet bulb to the surface of the acid. No difference, however, could be perceived from such a change at the full atmospheric pressure. I also ascertained that no change of relative position in the surfaces of the acid and water produced any alteration in the dew point under any circumstances.

The few simple facts above determined appear to me to be intimately connected with the solution of some very important atmospheric phenomena, and I shall endeavour briefly to indicate their relation.

The aqueous fluid is so abundantly spread over the face of the earth, that there can be no doubt that the permanently-elastic atmosphere, which surrounds it, would very speedily be saturated with its steam, did not some cause, analogous to the sulphuric acid

in the receiver, prevent its universal diffusion. This never-failing cause is inequality of temperature. As in the small experiment we found that the degree of dryness was proportioned to the energy of the absorbent mass, and that the existing vapour was equally diffused between it and the exhaling surface; so, in the larger operations of nature, we shall find that the state of saturation is dependant upon the point of precipitation, and that the aqueous atmosphere is nearly uniform between it and the source of steam.

Now, it is well understood that the temperature of the gaseous atmosphere in its natural state must decrease with its density as we ascend to its upper parts; so that a great degree of cold is at all times to be found within a very moderate distance from the surface of the waters. It is this low temperature which determines the tension of the aqueous atmosphere, and it is evident that the evaporation which is thus caused at the base of the aerial fluid, must be accompanied by a simultaneous and equal precipitation above. What then becomes of the precipitated moisture? Let us endeavour to trace the order of this phenomena. We will first suppose a calm state of the atmosphere, a temperature of 80°. and the barometer at 30 at the surface of the earth. By a calm state of the atmosphere is here meant, one that is free from any lateral wind, and in which, the only currents being in an ascending and descending direction, evaporation would proceed at the rate exhibited in the first column of Mr. Dalton's table. The dew-point at the surface of the earth is 64°, and this is determined by the temperature at the height of about 5000 feet, where the barometric column would maintain itself at 24 inches. The degree of saturation below would therefore be 600, and the amount of evaporation 1·74 grains per minute from a surface of six inches diameter. This quantity we therefore suppose condensed at the height before named. But the state of saturation in the atmosphere, above this point of precipitation, is again diminished; for we may suppose the force of the vapour to be determined by a temperature of 31° at a height of 15,000 feet, where the barometer would stand about 16 inches. The force of evaporation would, therefore, be 1.71 grains per minute, at the full atmospheric pressure; and this amount

increasing as the pressure diminishes, would give 2·13 grains per minute; so that the power of evaporation at this stage exceeds the supply of moisture, and no cloud could possibly be formed. Above the second point of condensation let us now suppose the force of the vapour to be determined, in still loftier regions, by a temperature of 12°. The force of evaporation would then be 0·44 grains, increased in the proportion of 16 inches to 30, or 0·82 grains. Here, then, the power of evaporation would be insufficient to diffuse in the upper regions the whole of the moisture supplied from the surface of the earth, and a cloud, it might be supposed, must consequently result. But another modification of the process now ensues; the precipitated moisture has a tendency to fall back into the warm air below it, and consequently would again assume the elastic form with a rapidity proportioned to the rarefaction of the stratum in which it is diffused. There is, I think, no difficulty in supposing that no visible cloud, or one of extreme tenuity, would be formed during this double process of evaporation. A very important re-action, however, must take place upon the strata of vapour beneath; the elastic force being increased above, enables the water below to maintain an atmosphere of a higher degree, and the quantity of evaporation must decrease as the point of saturation rises. A different arrangement of the points of precipitation would ensue in the progress of these effects.

An important distinction must here be drawn between the ultimate effects of the superior and inferior evaporation denoted above. In the first, the whole weight of water is condensed and simultaneously exhaled; and although it constitutes steam of an inferior degree of force, there is little or no difference in the quantity of its latent heat, and no effect is therefore produced upon the temperature of that portion of the atmosphere in which the change takes place. But in the second, the condensation happens at one spot, and the vaporization at another inferior to it; the latent heat is therefore evolved at the former and communicated to the air, while at the latter the process is reversed, and the air is cooled. The process of this operation would, therefore, tend to equalize the temperature of the atmosphere.

We will next imagine that the surface of the earth is swept by a high wind, and that the atmosphere instead of resting calmly upon its base, moves laterally with great velocity. Under these circumstances experience has shewn that the amount of evaporation will be nearly doubled; but the force of evaporation is not altered in the upper regions. The inferior exhaling surface being immoveable, the motion of the air perpetually changes, and renews the points of contact, and prevents accumulation at any one place; but in the heights of the atmosphere the exhaling surface of the cloud is borne upon the wind, and their relative situations never change.

The progress of precipitation must, therefore, necessarily, under these circumstances, outstrip that of evaporation, and the disturbance of the atmospheric temperature will be greatly accelerated.

There is another cause which would also quicken evaporation below, without equally increasing its power of diffusion at any given height above; and that is a decrease in the density of the air at the surface of the earth. Under the circumstances of our first supposition imagine the barometer to fall to 28 inches, the evaporation would be increased from 1·74 grains per minute, to 1·86 grains; but this decline of two inches at the surface would indicate a contemporaneous fall of little more than one inch at the height of 15,000 feet, and the rate of diffusion would vary accordingly. When it is considered that great falls of the barometer are generally accompanied by high winds, and that this disparity is multiplied by the force of the current, it is easy to appreciate the influence of this local increase of the power of evaporation.

The facility of evaporation in the rarer regions of the atmosphere will also go far to account for the state of saturation in which the air of mountainous countries is generally found, and many minor meteorological phenomena might probably meet with their explanation from variations of the same cause; such as the fogs which frequently accompany a very high degree of atmospheric pressure, and that peculiar transparency of the air which often precedes rain, and is accompanied by a falling barometer. But to return

again to the more general and extended influence of the vapour upon the boundless strata of the atmosphere:—that the phenomena of evaporation and condensation, as we have been contemplating their progress, have not been described with any bias to theoretical considerations, but are in strict accordance with facts and observations, any one might easily convince himself with less difficulty than would at first be supposed. To prove the assertion I shall extract the following passages from the works of De Luc, who was probably one of the most accurate observers of nature that ever existed, and who seldom, indeed, allowed any hypothetical considerations to warp his description of what he had observed. They will afford a complete illustration of the preceding remarks, although they were penned by him to support a very different hypothesis.

"Si l'on ne fait qu'une légère attention à la surface de ces brouillards vus des montagnes pour en jouir comme d'un beau spectacle, on peut penser qu'ils sont permanens; que l'évaporation est arrivée à son maximum à la surface des eaux, parce que l'air est parvenu à l'humidité extrême; et que les vapeurs vesiculaires qui troublent la transparence de cet air restent les mêmes durant des semaines ou même des mois; cest-a-dire, tant que le brouillard se conserve à une même hauteur. Mais le phénomène diffère beaucoup de cette première apparence: l'evaporation continue à la surface des eaux, les vapeurs vésiculaires qui s'en forment montent sans cesse et une nouvelle évaporation a lieu à la surface des brouillards. C'est un spectacle aussi amusant qu'instructif, que celui que fournit cette surface, vue d'un lieu peu élevé audessus d'elle, et dans une grande vallée ou l'on ait à quelque distance, des montagnes rembrunies par des forêts de sapins. Une telle vallée éclairée par les rayons du soleil semble être comblée de coton, filé dans toute sa surface par des êtres invisibles en fils invisibles: il s'y fait par-tout des tumeurs, semblables a celle que produit une fileuse sur sa quenouille en tirant le coton pour former son fil, et elles disparoissent successivement en se dissipant dans l'air. Quelquefois ces tumeurs s'allongent et se séparent de la masse en tendant à monter: on les voit alors

s'étendre comme un paquet de gaze qui se déploie et peu à peu elles disparoissent. Les brouillards se forment donc constamment à la surface des eaux et du sol; mais constamment aussi ils se dissipent dans l'air supérieur: et cependant on n'apperçoit point que l'humidité y augmente."—*Idées sur la Météorologie*, Tom. II, p. 78.

"Depuis que mes idées ont changé sur la cause de la pluie, j'ai fort souvent fixé mon attention sur les nuages et j'ai reconnu trés évidemment, qu'ils s'évaporent même tandis qu'ils grossissent. Si l'on fixe ses regards sur leur bord découpé qui, lorsqu'il a pour fond l'azur du ciel, presente mille figures singulières, celles que l'imagination leur prête alors, peut aider à l'examen dont je parle, en rendant leurs changemens plus frappans. Il arrive souvent, que la partie sur laquelle on fixe son attention se dissipe au lieu même où l'on a commencé à l'observer: souvent aussi on la voit s'étendre, sans que la totalité du nuage se meuve, et elle ne se dissipe pas moins durant cette extension. Quelquefois, tandis que l'un des festons du nuage se dissipe on en voit d'autres se former, s'étendre, produire eux-mêmes de nouveaux festons; par ou le nuage grossit: d'autres fois il diminue; et alors tous ses festons s'évaporent successivement et il n'en acquiert de nouveaux, que parcequ'il se découpe: on apperçoit en même tems, qu'il devient plus mince et il disparoit enfin totalement.

"C'est ce qui m'a conduit à penser qu'il y a en effet dans l'air, une source générale de vapeurs qui en fournit en certaines circonstances; que ces vapeurs sont produites au lieu même ou se forme un nauge; que c'est par la durée de cette production de vapeurs, que les nuages subsistent, s'aggrandissent même, quoiqu'en s'évaporant tout le tour; et que lorsqu'ils se dissipent c'est que leur evaporation n'est plus réparée par la formation de nouvelles vapeurs."—*Ib.* p. 117.

I shall now conclude this paper with an observation which is intimately connected with the subject of the preceding pages. It has been argued that the quantity of heat which would be communicated to the air by the condensation of atmospheric vapour would be trifling, and inadequate to produce those expansions in

the aërial currents to which, in my essay upon the constitution of the atmosphere, I have ascribed the fluctuations of the barometer. Now, I have therein shewn how the gradual spread of a small increase of temperature, through a considerable stratum, is sufficient for the purpose; and a very little consideration will, I think, convince any one that the evolution of caloric is by no means so small as has been supposed.

The following rough calculation will place the facts in a striking point of view:—The latent heat of steam has been proved to be somewhere about 970°, and it is known that, whatever be its density, or the temperature at which it is produced, the amount will differ but little from this estimate. The condensation, therefore, of a pound of steam of any degree of elasticity would be adequate to raise a pound of water 970°. The capacity of atmospheric air, of mean density, for heat, compared to that of water, is as ·2669 to 1; therefore the same quantity of heat which would raise a pound of water 1°, would raise a pound of air 3°·7. The condensation of a pound of steam would, therefore, elevate the same weight of air to 3589°. A pound of air is equal to about 11 cubic feet, so that the evolution of heat from the condensation of a pound of steam, would be sufficient to raise the temperature of 3657 cubic feet of air 10°.

When we now look to the depth of water which falls upon the surface of the earth, and recollect that this is not the sole measure of the effect we are endeavouring to estimate, but that the unceasing precipitation and exhalation of the clouds is perpetually extending this influence to the most inaccessible heights, we shall, perhaps, have a juster notion of the prodigious power of atmospheric vapour; and it will, I think, be granted that I have not over-rated the impulse which it is calculated to impart.

ART. VII.—*A Design for making a Public Road under the Thames, from the east side of the Tower, near Iron-Gate Stairs, to the opposite side of the River, near Horseley-down Stairs.* By Samuel Ware, Esq.

[Communicated by the Author.]

Internal Dimensions of the Arch-way.	The carriage-road . . .	28 feet wide.
	The height above the road .	18 feet.
	The foot-paths	14 feet wide.
	The greatest width . . .	42 feet.
	The greatest height . . .	21 feet.

The following particulars of the ESTIMATE describe the mode of erecting the arch-way:—

Compensation for the ground and buildings on the north side of the river, and for the ground and buildings on the south side, to form the approaches; cofferdams, in ten successive lengths or removes, to keep out the water; and strutting, to keep up the ground.

Steam-Engines, to keep the works within the cofferdams dry, and subsequently for draining the road, should there be occasion.

Digging out a channel, in the bed of the river, for the arch-way, and the ground for the approaches.

Removing the refuse earth; claying, filling in, and leveling, two feet above the extrados of the arch.

Yorkshire Ledgers for the foundations of the arch-way, and walls of the approaches and embankments, and piling as occasion may require.

Stone-work, cut in voussoirs, of the arch, and counter-arch.

Lining with lead, 10*lb.* to the foot superficial, enveloping these arches.

Super-arch of brick-work, lined externally with tiles in cement.

Centering for the arches.

Forming and gravelling the road, ascending one foot perpendicular to twenty feet horizontal.

Drains, pipes, foot-paths, and lamps.

Embankments, and other walls and parapets, in the approaches *.

Facings to the entrances to the arch-ways, and toll-houses.

Estimated amount of the above works £250,000

Pecuniary Advantages.

A small part of such a revenue †, as would be derived from the number of passengers and carriages which has been estimated to pass London Bridge daily, calculated at the tolls allowed to the Southwark Bridge Company, would be ample to compensate the cost of this arch-way.

The taxes arising out of the materials of the buildings likely to be erected, together with the assessed taxes arising out of them when built, in the ways to and through the lower road to Deptford, and in communicating with the Kent Road, consequent on such a connexion between the two sides of the Thames, would be a great source of revenue to the government, probably more than sufficient to compensate the cost of this arch-way.

* This mode of approach is also applicable to a road descending from a bridge; is cheap, by lessening the expense of compensation for the buildings and ground required and damaged in making an inclined plane; and is convenient, because the foot of the inclined plane is at the river.

		BRIDGES.			Average	Toll	£.	s.
		London	Blackfriars	Westminst.		d.		
† Foot Passengers - -	Passing Daily [a]	89640	61069	37820	62813	1	261	17
Waggons - - - - - - -		769	533	173	492	12	24	12
Carts and Drays - - -		2021	1502	963	1796	8	59	17
Coaches - - - - - - -		1240	990	1171	1133	9	42	9
Gigs and Taxed Carts		485	500	569	518	4	8	12
Horses not drawing -		764	522	615	633	1½	3	19
							£401	6 [b]

a See Month. Mag. March, 1816, and Morn. Chron. May 26, 1812.

b £400 per day, or £146,000 per annum.

The saving in time, and in the wear and tear of carriages, horses, and men who would otherwise go over London Bridge, or cross the river in boats, would be a compensation for the tolls to be paid at this arch-way.

The carriages and passengers are those coming from the streets adjacent to the site of the proposed arch-way, those going into or through Surrey from the East India, West India, and London Docks, from the Commercial Road, and from the Counties of Hertford, Cambridge, Norfolk, Suffolk, and Essex; also those going to the above-mentioned places from the counties of Surrey and Kent.

The necessity of increasing the width of the intended new London Bridge, by this diminution of the number of passengers and carriages, may be obviated, and a large sum of money thereby saved.

Political Advantages.

The communications, by this road, between the officers of government, and the Mint, Trinity-House, Custom-House, and the Tower, may be facilitated.

A readier transfer of soldiers, arms, and stores, to and from the counties north and east of London, and the Tower, to and from Woolwich, Chatham, and Sheerness, by land, will be obtained by this arch-way.

This arch-way may be made a military pass, there being proposed a private way to it from the Tower.

Observations.

It seems remarkable, considering the great advantages to be obtained in populous cities by opening a communication between the shores of a navigable river, for foot-passengers, horses and carriages, without interrupting the navigation on the river, that the passage under the Euphrates, constructed by Semiramis, at Babylon, is the only one upon record. The account Diodorus the Sicilian gives of it may be translated thus:—

"In the low ground of Babylon, Semiramis sunk a square pond, 35 feet deep, each side being 300 stadia in length, the banks

whereof were lined with bricks well cemented with bitumen *, she then turned into it the water of the Euphrates †. Across the channel of the river, thus made dry, she then made a passage in the nature of a vault from one palace to the other. The arch was built four cubits ‡ thick, of firm and strong bricks, plastered all over on both sides with bitumen. The walls supporting the arch were 20 brick § in thickness, and 12 feet high from the floor to the springing of the arch, and the breadth of the passage was 15 feet.

* Dr. Hulme, (Archæologia, vol. xiv. page 57,) analyzed the cement adhering to a brick brought from Babylon, and found it to be bitumen.

† Strabo, (lib. xvi. page 738,) states the width of the Euphrates to be one stadium, which is generally taken at a furlong, or 660 feet. M. Gosselin shows that there were two stadia; one used by Herodotus, called the short stadium, about 329 feet English: the other of Archimedes, about 438 feet English. Ctesias, from whom Diodorus had his account, used the stadium of Archimedes. Herodotus used the short stadium. In this way the discordance of Herodotus and Ctesias, in respect to the wall of Babylon, has been reconciled.

‡ A cubit royal of Babylon was estimated, by Romé de Lille, at $22\frac{2}{10}$ inches English.

§ There is a brick in the British Museum, brought from the site of ancient Babylon. That described by Dr. Hulme, in Archæologia, vol. xiv. page 55, is $13\frac{1}{2}$ inches square, and 3 inches in thickness, and weighs 38 lb. 11 oz. avoirdupois. He analyzed the material, and found it to be pure clay, and not burnt. Dr. Henley, in the same volume, page 205, deciphered the inscription on it, "a brick baked by the sun." Pocock measured some of the bricks of the brick Pyramid at Saccara, built by king Asychis; he found some $13\frac{1}{2}$ inches long, $6\frac{1}{2}$ broad, and 4 thick; and others 15 inches long, 7 broad, and $4\frac{3}{4}$ thick.

In Rennel's Geo. Sys. of Hero, section 11, page 356, is the following note: "Diodorus describes a vaulted passage under the bed of the Euphrates, by which the Queen Semiramis could pass from one palace to the other, on different sides of the river, which was a stadium in breadth, (according to Strabo, page 738,) without crossing it. This serves, at least, to show, that the palaces were very near the river's banks."

"At a time (1800) when a tunnel, of more than half a mile in length, under the Thames (at Gravesend) is projected, it may not be amiss to mention the reported dimensions of the tunnel made by Semiramis, under the Euphrates; which, however, was no more than 500 feet in length, or less than 1-5th of the projected tunnel under the Thames. That of Semiramis was said to have been 15 feet in breadth, 12 feet in height to the springing of the arch, perhaps 20 in all. The ends of the vault were shut up with brazen gates. Diodorus had an

This work was finished in 260 days, and then the river was turned into its ancient channel; so that Semiramis could go privately from one palace to another, under the river. She made also two brazen gates at each end of the vault, which continued to the time of the kings of Persia, the successors of Cyrus."

In 1798, a tunnel, 900 yards in length, was projected to pass under the Thames, to unite Tilbury, in Essex, with Gravesend, in Kent, at an estimate of only £15,955. Subscribers were obtained to promote the undertaking, by whose means an engine-house and steam-engine were erected, and a shaft dug, about 146 feet deep, when the engine-house was burnt, and the operations were abandoned.

In 1805, an Act of Parliament (45 Geo. 3. cap. cxvii.) was obtained, to construct a tunnel under the Thames, at the Old Horse Ferry, about 2¾ miles below London Bridge, and to raise £140,000, and a further sum of £60,000, in all £200,000. A shaft, 76 feet deep, was sunk, and a driftway, 5 feet high, 3 feet wide at the bottom, and 2 feet 6 inches at the top, was extended, under the direction of Mr. Trevetheck, a Cornish miner, 1011 feet from the south shore, under the bed of the Thames, when sand and water burst in upon the workmen, and further progress was suspended. The powers given by this act are now, by lapse of time, void. In 1809, notice was given,

idea* that the Euphrates was 5 stadia in breadth, see lib. ii. c. 1. The Euphrates was turned out of its channel, in order to effect this purpose. Herodotus, who is silent concerning the tunnel, says, that the river was turned aside in order to build a bridge. Diodorus describes a bridge also. There is an absurd story told, in both these historians, respecting the disposal of the water of the river during the time of building the bridge. According to them, the water was received into a vast reservoir, instead of the obvious and usual mode of making a new channel to conduct the river, clear of the work constructing in its bed, into the old channel, at a point lower down †."

* Diodorus merely states, that the bridge built by Semiramis was 5 stadia in length. Bridges are frequently five times as long as the width of the river they stride.

† This story, from the vastness of the reservoir, may be true. Local circumstances may have compelled Semiramis to adopt this apparently extravagant mode of removing the water of the Euphrates from the site of the tunnel.

by public advertisement, that the directors were desirous of receiving designs for proceeding again in this work, and they offered a premium of £200 for the plan which should be adopted, and a further premium of £300 upon the execution of it. Since that time the project has lain dormant. Lately a pamphlet has appeared, entitled "A New Plan of Tunnelling, calculated for opening a Road-way under the Thames, by M. T. Brunel, Esq., in order to the raising a capital of £——— by transferable shares of £100 each," for commencing again this project. Mr. Brunel describes his plan as follows: "The character of the plan before us consists in the mode of effecting this excavation by removing no more earth than is to be replaced by the body of the tunnel, retaining thereby the surrounding ground in its natural state of density and solidity."

Mr. Brunel proposed that the excavation, 34 feet wide by 18 feet high (external dimensions), consisting of 33 such drift-ways as that before mentioned, moving simultaneously, worked by 33 men, at the rate forward of three feet per day, followed by a brick tunnel at the same pace, should pass in a stratum which he states, "has been found to resist infiltrations," so that the crown of the tunnel will have a head of earth on it, from 12 to 17 feet in thickness, quite undisturbed, as he expects.

The method proposed by Mr. Dodd, at Gravesend, by Mr. Vazie, at Rotherhithe, and by Mr. Brunel, is by mining. Other methods may or have been proposed, such as to dredge out a channel in the bed of the river by machinery in vessels, and afterwards to sink therein caissons with brick or stone tunnels in them, to be afterwards secured together and perforated; or to sink large iron cylinders or boxes, the size of the proposed tunnel, with moving, lapping, and closing joints, let down, one after another, on strong iron mooring chains, into the channel so dredged out; the junctions to be facilitated by means of the diving-bell: but these schemes are of a very adventurous character, and might be tried perhaps with propriety in the case of a small passage under a river. The apparent cheapness of such methods seems calculated

to obtain subscribers to such a project, but not to effect a dry and secure passage for men and carriages under the Thames.

The method of Semiramis was simple in design and certain of success. Troughs holding water, such as the canal aqueducts over rivers, are to be seen in all parts of the country; and there can be no doubt, that such an arch-way as that before described* in the estimate commencing this statement, executed in the open air, and uninterrupted by water during such erection, by means of cofferdams, would have a successful issue, and be perfectly dry under a river, for a thoroughfare for passengers and carriages. In modern times, a cheaper way of rendering the bed of a river dry has been discovered than that of Semiramis, which was by means of a reservoir to receive the waters of it, or even than that of Trajan, in building the bridge across the Danube, which was by making a temporary new channel to receive its stream. We have lately seen the piers of Waterloo Bridge and of Southwark Bridge laid dry, in the bed of the Thames, by means of cofferdams, the use of which, in keeping the space enclosed in them free from water, has been greatly extended by the facility obtained by means of steam-engines; and by similar means may an arch-way be constructed of almost any useful dimensions under the river, and with the same success, and not with more interruption to the navigation of the Thames than would be caused by one of the vessels in the pool getting athwart the stream, and remaining so for a few months.

Since the foregoing statement was made, an advertisement has appeared of the intention of applying to Parliament for leave to bring in a bill to erect a patent wrought-iron bar bridge of suspension, from some part of the parish of St. Botolph, Aldgate, over the Thames, to some part of St. Mary, Bermondsey, of such

* The following account of the suspended gardens of Nebuchadnezzar, at Babylon, extracted from Diodorus, will show the care used by him, to render the rooms under them dry. "On the walls were laid stones, 16 feet long and 4 feet broad; these were covered with reeds coated with brimstone, on which were laid double tiles, cemented together, and on them were laid sheets of lead."

a height as to admit vessels to pass under it at spring tides, without lowering their masts.

These repeated attempts to obtain a road-way for passengers and carriages eastward of London Bridge, across the river, together with the almost impassable state of London Bridge, from the crowds on it, in the middle of the day, show that there is a demand for such a communication between the sides of the Thames, eastward of London Bridge. The questions to be considered, are, First, what method of obtaining such an object is the best? Secondly, how that method can be carried into effect with certainty? Thirdly, what is the best site for such a road-way? Fourthly, whether such a work, in the site hereby laid down, would not be of such political importance, facilitated as the execution thereof would be by a possession of the ground necessary for the northern approach, as to warrant the State in undertaking the work, leaving to the public the use, subject to certain tolls and restrictions, as may accord with the uses of it by Government? Fifthly, as to time, should the cofferdams necessary to resist the deep water in the erection of the new London Bridge be of such a size as to cause an impetus to the river, or alteration of the mid-stream, so as to destroy the present bridge, or render it impassable, (it being intended that it shall remain until the new one is passable), would not then such a way as the one hereby proposed be a great relief to Southwark Bridge, until a temporary bridge be supplied? And, comparing generally the expediency and cost of carrying into effect this design with the expediency and cost of rebuilding London Bridge *, ought not this work to have the precedence?

SAMUEL WARE.

5, John Street, Adelphi.

* See this Journal of Science, Roy. Inst., Nos. 29, 30, and 31, 1823; and Tracts on Vaults and Bridges, 1822.

Art. VIII. *An Account of the Overflowing Well in the Garden of the Horticultural Society at Chiswick.* (*Communicated by* Joseph Sabine, Esq., S. H. S. *&c.*)

[The specimens adverted to in the following paper are deposited in the Mineral Room, at the Royal Institution.]

In consequence of the success which had attended the operations of several persons in the vicinity of Chiswick in boring for water, it was determined by the Council of the Horticultural Society that an attempt to procure an overflowing well should be made in the society's garden, for the purpose of obtaining a supply of water for various purposes; but more particularly to form an ornamental canal in the Arboretum for the growth of hardy aquatic plants.

After the necessary inquiries had been made, it was determined that Mr. John Worsencroft, a person who had previously succeeded in making an overflowing well for Messrs. Bird, of Hammersmith, should be employed to execute the experiment. He commenced his operations upon the first of September last; and after boring for five weeks without material interruption, tapped the spring on the 18th of October, and finally completed his task on the following day. The depth from which the water first rose was 317 feet, and the whole depth of the well, when completed, was 329 feet; the additional 12 feet of boring having been made in order to gain a perfect opening into the bed of the spring, which flowed when first tapped less copiously than after the final depth was obtained. The chalk from which the water immediately comes is soft, but the bottom of the well is in hard chalk. The water in all the neighbouring wells appears to have been obtained at about the same depth; and the strata through which the perforations were made are nearly similar to those met with in the present instance.

The tackle and instruments used were very simple. A scaffolding was raised 20 feet above the proposed orifice of the well, on which a platform was fixed to support a windlass, by which the

rods used in boring were lowered into, and raised from, the well. These rods were of tough iron, about an inch and a half square, and ten feet long; the ends of each screwing on to, or unscrewing from, the top of the next, as they were lowered into, or raised from, the hole. The instruments fixed as occasion required to the lowest extremity of the series of rods when in action, were augers of various dimensions for boring, steel chisels for punching, and a hollow iron cylinder, (called a shell,) fitted with a valve at its lower end, for bringing up soft mud. The rods, when an auger was attached to them, were turned round by means of moveable arms or dogs, which were made to lay hold of the part of the uppermost rod at the top of the hole; the auger being thus forced through the stratum of clay or sand, was drawn up as soon as its cavity was filled with the substance it had loosened. The chisels were employed for punching through stones, hard substances, or hard chalk; the rods, when these were attached, were moved by means of a powerful beam acting as a lever, and worked by four men.

The water is discharged at the surface of the ground after the rate of six gallons per minute, and is capable of being carried 20 feet above the ground level; and even then supplies a copious stream. The well is lined for the first 186 feet with cast-iron pipes, with a three-inch bore, jointed by means of wrought-iron collars, which are rivetted into the pipes; the succeeding 77 feet 6 inches are lined with copper pipes, with 2½ inches bore, soldered into a single length, and resting in the chalk, through which the remainder of the hole is bored, and in which no pipes were used. The whole series of pipes was introduced at once, the hole having been prepared for receiving them as soon as it was ascertained that the augers had reached the chalk stratum. The land springs in the gravel, above the blue clay, were kept out in the first instance by extra iron pipes. The spring which was found in the sand below the blue clay, and above the chalk, rose to within a few feet of the surface, but did not overflow. The whole of the water of this spring is, however, excluded from the well by the pipes with which it is lined.

The cost of the well, including that of the pipes, boring, and every other expense whatever, did not exceed 130*l.*; and the manner in which it was executed, was, in every respect satisfactory. Indeed it is impossible to speak too highly of the care, attention, and dexterity of Mr. Worsencroft, and the workmen whom he employed.

Turnham Green, November 27, 1823.

Memoranda of the various strata bored through.

Feet.

19 Gravel.

162 Blue clay. *Specimen No.* 1. At the depth of 59 feet from the surface a small stone, *sp. No.* 2, four inches thick was found; another at 149 feet was found six inches thick, but it was pounded to dust by the chisel; a third was found at the depth of 153 feet; it is marked *sp. No.* 3. At 162 feet from the surface the clay became veiny, and intermingled with very minute glittering fragments; this is *sp. No.* 4. At 173 feet the clay became more sandy, *sp. No.* 5, and continued so till it altered into the next kind.

30 Coloured clay; varying from brick-red, mixed with blue and yellow, to many shades of dull purple; *sp.* 6, came from 190 feet; *sp.* 7, from 200 feet; *sp.* 8, from 203 feet; *sp.* 9, from 211 feet, when the seam changes into the next which is more yellow.

22 Clay, with nearly an uniform colour of yellow ochre, occasionally mixed irregularly with grey. *Sp.* 10. This was more sandy than the previous stratum. Among this water rose in some quantity.

28.6 Soft soil, apparently composed of clay and sand. It varied very much in colour, being sometimes bright green, otherwise yellow intermixed with green, or sometimes beautifully veined with dark red and yellow. Many specimens are sent, *viz.*:

sp. 11, 240 ft.	*sp.* 12, 242 ft.	*sp.* 13, 243 ft.
sp. 14, 244 ft.	*sp.* 15, 245 ft.	*sp.* 16, 246 ft.
sp. 17, 247 ft.	*sp.* 18, 255 ft.	*sp.* 19, 261.6 ft.

The last specimen is of the soil immediately above the chalk. Two stones were met with in this stratum; one like those formerly mentioned, of which no specimen could be preserved; the other a flint, *sp.* 20, at 257 feet.

67.6 Chalk; among which many flints were scattered. Of these, one, *sp.* 21, was one foot in thickness, and so unusually hard as to occupy the workmen three days in punching before they could force a way through it. The water was found at the depth of 317 feet, in a bed of soft chalk, mixed with small flints; the hole was bored 12 feet among the water, so that the total depth of the well is 329 feet; and it is supposed by the workmen that the last piece of chalk that was brought up sticking to their punch, was from the upper surface of a new layer of chalk in which there is no water. *Specimen* 22, is a morsel of a hard stone, apparently containing ore, which was brought up in the auger from among the chalk, at the depth of 274 feet. *Specimen* 23, is of the first chalk which was found at 261.6 feet. *Specimen* 24, is from 317 feet, when the first water was found; it was saturated with moisture when first brought up; *sp.* 25, is the last piece of chalk brought from 329 feet, and supposed by the workmen to be from the upper surface of a new and dry layer of chalk; *sp.* 26, various fragments of large flints broken by the punch at different depths in the ground; *sp.* 27, morsels of flint and pebbles washed out of the chalk raised from the water-source, and supposed not to have been broken in punching, but to have laid among the water in their present condition. In cutting a solid piece of chalk, which had been brought up in the auger, a morsel of flint, exactly like these specimens, was observed, with every appearance of not having been forced into its place in the chalk by violence.

The principal impurity discovered in this water by the action of reagents is common salt, of which it contains about four grains and a half in the pint. When evaporated to dryness, the residue

contains a sufficient quantity of carbonate of soda to render it very manifestly alcaline; this is also the case with the waters of the other deep wells in and about London.

J. L.

ART. IX. *On the Taylorian Theorem.*

[To the Editor.]

Trinity College, Dublin,
October 11, 1823.

Sir,

I lately communicated to you a demonstration of the Taylorian Theorem given to me at lecture in this University by Mr. Edward Wilmot, a gentleman-commoner, and under-graduate of this college. The same gentleman has since given me an extension of this to functions of several variables, which I now enclose. The simplicity of the proof, and the elementary nature of its principles, must render it very valuable to the student. You will observe that it is independent of the Theorem of Maclaurin, and gives it as a corollary. It is also free from the functional reasoning so objectionable in other proofs of this theorem.

I am, Sir, *&c. &c.*,

DIONYSIUS LARDNER.

Let $u = F(x, x', x'', \ldots\ldots)$, x, x', x'', *&c.*, being independent variables,

Let $u' = F(x + \Delta x, x' + \Delta x', x'' + \Delta x'', \ldots\ldots)$

And let this be supposed to be expanded according to the dimensions of $x + \Delta x$, *&c.*

$$\begin{aligned} u' = {} & A(x + \Delta x) + A'(x' + \Delta x') + A''(x'' + \Delta x'') \ldots\ldots \\ & + A_2(x + \Delta x)^2 + A_2'(x' + \Delta x')^2 + A_2''(x'' + \Delta x'')^2 \ldots\ldots \\ & + B''(x + \Delta x)(x' + \Delta x') + B'(x + \Delta x)(x'' + \Delta x'') + B(x' + \Delta x')(x'' + \Delta x'') \ldots\ldots \\ & + A_3(x + \Delta x)^3 + A_3'(x' + \Delta x')^3 + A_3''(x'' + \Delta x'')^3 \ldots\ldots \\ & + \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ & \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (A) \end{aligned}$$

Hence

$$\begin{aligned} u = {} & Ax' + A'x' + A''x'' \ldots\ldots \\ & A_2x^2 + A_2'x'^2 + A_2''x''^2 \ldots \\ & B''xx' + B'xx'' + Bx'x'' \ldots \\ & A_3x^3 + A_3'x'^3 + A_3''x''^3 \ldots \\ & \ldots\ldots\ldots\ldots \\ & \ldots\ldots\ldots\ldots \qquad (B) \end{aligned}$$

This being successively differentiated for each of the variables, gives

$$\begin{aligned} & \left\{\frac{du}{dx}\Delta x + \frac{du}{dx'}\Delta x' + \frac{du}{dx''}.\Delta x'' \ldots\ldots\right\} = \\ & = \Delta x \,\{A + 2A_2x + B''x' + B'x'' + 3A_3x^2 \ldots\ldots\} \\ & + \Delta x' \,\{A' + 2A_2'x' + B''x + Bx'' + 3A_3'x'^2 \ldots\ldots\} \\ & + \Delta x'' \,\{A'' + 2A_2''x'' + Bx' + B'x + 3A_3''x''^2 \ldots\} \\ & \ldots\ldots\ldots\ldots \\ & \ldots\ldots\ldots\ldots \qquad (C) \end{aligned}$$

$$\begin{aligned} & \left\{\frac{d^2u}{dx^2}.\frac{\Delta x^2}{1.2} + \frac{d^2u}{dx'^2}.\frac{\Delta x'^2}{1.2} + \frac{d^2u}{dx''^2}.\frac{\Delta x''^2}{1.2} \ldots\right\} = \\ & = \Delta x \,\{A_2 + 3A_3x \ldots\ldots\ldots\} \\ & \quad \Delta x' \,\{A_2' + 3A_3'x \ldots\ldots\ldots\} \\ & \quad \Delta x'' \,\{A_2'' + 3A''x \ldots\ldots\ldots\} \\ & \ldots\ldots\ldots\ldots \\ & \ldots\ldots\ldots\ldots \qquad (D) \end{aligned}$$

$$\begin{aligned} & \left\{\frac{d^2u}{dx\,dx'}.\frac{\Delta x.\Delta x'}{1} + \frac{d^2u}{dx\,dx''}.\frac{\Delta x.\Delta x''}{1} + \frac{d^2u}{dx'\,dx''}.\frac{\Delta x'\;\Delta x''}{1}\ldots\right\} = \\ & = \Delta x.\,\Delta x' \,\{B'' \ldots\ldots\ldots\} \\ & + \Delta x\;\Delta x'' \,\{B' \ldots\ldots\ldots\} \\ & + \Delta x'\;\Delta x'' \,\{B \ldots\ldots\ldots\} \\ & \ldots\ldots\ldots\ldots \\ & \ldots\ldots\ldots\ldots \qquad (E) \end{aligned}$$

The sum of the series (D) and (E) may evidently be expressed thus,

$$\frac{d^2u}{1.2}\left\{\frac{\Delta x}{dx} + \frac{\Delta x'}{dx'} + \frac{\Delta x''}{dx''} \ldots\right\}^2$$

And hence if the series (A) be arranged by the dimensions of the quantities, Δx, $\Delta x'$, $\Delta x''$, ... and the substitutions suggested by the series (B), (C), (D), (E), &c., being made, the result will be

$$u' = u + \frac{du}{1} . \text{S} . \frac{\Delta x}{dx} . + \frac{d^2u}{1.2} . \left(\text{S} \frac{\Delta x}{dx}\right)^2 + \frac{d^3v}{1.2.3} . \left(\text{S} . \frac{\Delta x}{dx}\right)^3 . . . .$$

In which the symbol S $\frac{\Delta x}{dx}$ signifies

$$\frac{\Delta x}{dx} + \frac{\Delta x'}{dx'} + \frac{\Delta x''}{dx''} . . . . . .$$

The meaning of the symbols $\frac{d^2u}{1.2}\left(\text{S}.\frac{\Delta x}{dx}\right)^2$ &c., is sufficiently obvious.

This becomes identical with Taylor's series when S $\frac{\Delta x}{dx} = \frac{\Delta x}{dx}$.

The series of Maclaurin may easily be inferred from it by supposing $x = 0$ and changing Δx into x.

Art. X. ASTRONOMICAL PHENOMENA arranged in Order of Succession, for the Months of April, May, and June, in the Year 1824.

(Continued from Page 297.)

APRIL.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.
1	Sun		0 43	4 38 N
	Mars . . .		12 11	2 8 N
	Juno . . .		14 18	2 19 S
	Venus . .		22 55	8 14 S
	Mercury .		0 13	0 46 S
2	Sun . . .		0 47	5 1 N
	Im. * . . .	7.8	10 47	or $10^h 3'$ MT.
	*'s R.A. $8^h 14'$		Decl. 21° 25' N. (0')	
	Em. . . .		11 33	or $10^h 19'$ MT. (4'N.)
	Mars . . .		12 10	2 16 N
	Juno . . .		14 17	2 11 S
	Venus . .		23 0	7 47 S
	Mercury .		0 20	0 5 N
3	Sun		0 50	5 21 N
	Im. * . . .	7.8	9 40	or $8^h 51'$ MT.
	*'s R.A. $4^h 9'$		Decl. 23° 36' N. (4'S.)	
	Em. . . .		10 31	or $9^h 42'$ MT. (0')
	Mars . . .		12 8	2 23 N
	Juno . . .		14 17	2 4 S
	Venus . .		23 4	7 20 S
	Mercury .		0 26	0 56 N
4	Sun . . .		0 54	5 47 N
	Em. 2 Sat.		11 48	or $10^h 55'$ MT. (98)
	Mars . .		12 7	2 29 N
	Juno . . .		14 16	1 56 S
	Venus . .		23 9	6 53 S
	Mercury .		0 33	1 47 N
5	Sun		0 58	6 10 N
	Moon . . .		6 0	21 11 N
	Im. * 1 . .	1.8	7 52	or $6^h 56'$ MT.
	*'s R.A. $6^h 3'$		Decl. 24° 2' N. (7'S.)	
	Em. * 1 .		8 44	or $7^h 48'$ MT. (1'S)
	Im. * 2 . .	7	8 47	or $7^h 51'$ MT.
	*'s R.A. $6^h 6'$		Decl. 24° 1' N. (2'S.)	
	Im. * 3 . .	7	9 18	or $8^h 22'$ MT.
	*'s R.A. $6^h 6'$		Decl. 23° 47' N. (12'S.)	
	Em. * 2 .		9 45	or $8^h 49'$ MT. (5N)
	Em. * 3 .		10 1	or $9^h 5'$ MT. (7'S)
	Im. * 4 .	8	10 3	or $9^h 7'$ MT.
	*'s R.A. $6^h 8'$		Decl. 23° 40' N. (14'S)	
	Im. * 5 . .	7.8	10 13	or $9^h 17'$ MT. (9'S.)
	Em. * 4 .		10 36	or $9^h 40'$ MT. (11'S.)
	Em. * 5 .		10 52	or $9^h 56'$ MT. (9'S.)
	Im. * . 6 .	8	11 20	or $10^h 24'$ MT.
	*'s R.A. $6^h 11'$		Decl. 23° 50' N. (5'N.)	
	Em. * 6 .		12 3	or $11^h 7'$ MT. (11'N)
	Mars . . .		12 5	2 36 N
	Im. Jupiter		12 28	or $11^h 32'$ MT.
	Im. * 7 . .	7	12 46	or $11^h 49'$ MT.
	*'s R.A. $6^h 15'$		Decl. 23° 32' N (2'S)	
	Im. * 8 . .	7	12 51	or $11^h 54'$ MT.
	*'s R.A. $6^h 15'$		Decl. 23° 25' N. (1'S)	
	Im. * 9 . .		12 58	or $12^h 1'$ MT.
	*'s R.A. $6^h 15'$		Decl. 23° 18' N. (cont.)	
	Em. Jupr.		12 22	or $12^h 25'$ MT.
	Em. * 7 .		13 35	or $12^h 38'$ MT. (4 N)
	Em. * 8 .		13 36	or $12^h 39'$ MT. (5'N)
	Juno . . .		14 15	1 49 S
	Venus . .		23 14	6 37 S
	Mercury .		0 40	2 40 N
6	Sun		1 1	6 33 N
	Moon . . .		7 3	22 6 N
	Mars . . .		12 4	2 12 N
	Juno . . .		14 15	1 41 S
	Venus . .		23 18	6° 0' S
	Mercury .		0 48	3 34 N
7	Sun		1 5	6 55 N
	Moon . . .		8 4	18 30 N
	20 Canc. .	6	8 13	18 53 N
	θ	5·6	8 22	18 41 N
	VIII. 112 .	8	8 28	19 53 N
	Im. * 1 . .	7	8 40	or 7 36 MT.
	*'s R.A. $8^h 4'$		Decl. 18° 12' N. (16'S.)	
	Em. * 1 .		9 13	or $8^h 9'$ MT. (11'S.)
	Im. * 2 . .		9 20	or $8^h 16'$ MT.
	*'s R.A. $8^h 6'$		Decl. 18° 5' N. (14'S.)	
	Em. 3 Sat.		9 38	or $8^h 31'$ MT. (+100)
	Em. * 2 .		10 7	or $8^h 16'$ MT. (5'S.)
	Em. 1 Sat.		11 15	or $10^h 11'$ MT. (+100)
	Mars . . .		12 3	2 48 N
	Juno . . .		14 14	1 34 S
	Venus . .		23 23	5 33 S
	Mercury .		0 55	4 27 N
8	Sun		1 9	7 18 N
	Moon . . .		9 3	13 38 N
	IX. 55 . .	7.8	9 12	13 51 N
	Im. 1 Sat.		9 13	or $8^h 4'$ MT. (+100)
	IX. 81 . .	7.8	9 17	15 4 N
	IX. 120 . .	7·8	9 25	13 26 N
	Mars . . .		12 1	2 54 N
	Em. 4 Sat.		12 6	or $10^h 58'$ MT. (+101)
	Juno . . .		14 13	1 27 S
	Venus . .		23 27	5 5 S
	Mercury .		1 3	5 22 N

APRIL.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
9	Sun		1 12	7 40 N
	IX. 202 . .	8	9 45	8 54 N
	π Leonis .	4·5	9 51	8 53 N
	Moon . . .		9 59	7 52 N
	43 Leonis	6	10 14	7 26 N
	Mars . . .		12 0	3 0 N
	Juno . . .		14 12	1 19 S
	Venus . .		23 32	4 38 S
	Mercury .		1 10	6 17 N
10	Sun		1 16	8 2 N
	Mercury .		1 17	7 12 N
	55 Leonis	6	10 47	1 40 N
	Moon . . .		10 54	1 35 N
	65 Leonis.	5.6	10 58	2 55 N
	69 Leonis.	5.6	11 5	0 53 N
	Mars . . .		11 59	3 5 N
	Juno . . .		14 12	1 12 S
	Im. * . . .	6.7	16 33 or 15^h 16′ MT.	
	*'s R.A. 11^h 6′ Decl. 0° 17′ S. (14′ S.)			
	Em. * . .		17 17 or 15^h 59′ MT. (3′S.)	
	Venus . .		23 36	4 10 S
11	Sun		1 20	8 24 N
	XI. 148 . .	6·7	11 35	5 42 S
	XI. 167 . .	6	11 42	4 21 S
	Moon . . .		11 48	4 49 S
	XI. 221 . .	7.8	11 55	4 30 S
	Mars . . .		11 58	3 10 N
	Juno . . .		14 11	1 5 S
	Venus . .		23 41	3 42 S
12	Sun		1 23	8 46 N
	Mars . . .		11 57	3 15 N
	Moon . . .		12 43	10 58 S
	49 Virg.	5.6	12 59	9 48 S
	52 Virg. .	8	13 2	9 10 S
	XIII. 25 .	7.8	13 6	10 25 S
	Juno . . .		14 10	0 57 S
	Venus . .		23 45	3 15 S
13	Sun		1 27	9 8 N
	Mercury .		1 32	9 4 N
	Im. * . . .	6.7	8 39 or 7^h 11′ MT.	
	*'s R.A. 13^h 31′ Decl. 15° 33′ S. (16′S.)			
	Em. * . .		9 19 or 7^h 51′ MT. (8′S.)	
	Mars . . .		11 55	3 20 N
	75 Virg. .	6	13 23	14 27 S
	XIII. 139	8	13 28	15 33 S
	83 Virg. .	6	13 35	15 17 S
	Moon . . .		13 39	16 18 S
	Juno . . .		14 9	0 50 S
	Venus . .		23 50	2 47 S
14	Sun		1 31	9 30 N
	Mercury .		1 40	9 59 N
	Em. 3 Sat.		10 50 or 9^h 18′ MT. (+100.)	

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
	Mars . . .		11 54	3 24 N
	Juno . . .		14 9	0 43 S
	XIV. 116.	7	14 25	19 49 S
	Moon . . .		14 36	20 42 S
	10 Libræ .	7	14 42	17 37 S
	Im. * . . .	7	14 45 or 13^h 12′ MT.	
	*'s R.A. 14^h 37′ Decl. 20° 35′S. (cont.)			
	XIV. 212.	6	14 47	20 36 S
	Im. * . . .	7.8	19 59 or 18^h 25′ MT.	
	*'s R.A. 14^h 47′ Decl. 21° 26′N. (5′N.)			
	Em. * . .		20 48 or 19^h 14′ MT. (10′N)	
	Venus . .		23 54	2 19 S
15	Sun		1 34	9 51 N
	Mercury .		1 47	10 54 N
	Mars . . .		11 53	3 30 N
	Juno . . .		14 8	0 36 S
	Im. * . . .	6	14 16 or 12^h 40′ MT.	
	*'s R.A. 15^h 34′ Decl. 23° 50′ N. (2′S.)			
	Em. * . .		15 24 or 13^h 47′ MT. (6′N.)	
	42 Libræ .	5.6	15 30	23 14 S
	Moon . . .		15 35	23 50 S
	XV. 192 .	6	15 43	23 27 S
	XV. 213 .	7.8	15 48	23 1 S
	Im. * . . .	6	20 4 or 18^h 27′ MT.	
	*'s R.A. 15^h 43′ Decl. 24° 0′ N. (cont.)			
	Venus . .		23 59	1 51 S
16	Sun		1 38	10 12 N
	Mercury .		1 55	11 50 N
	Mars . . .		11 52	3 31 N
	Im. * . . .	7.8	13 49 or 12^h 9′ (MT)	
	*'s R.A. 16^h 29′ Decl. 25° 42′ S. (15′S.)			
	Em. * . .		14 2 or 12^h 22′ MT. (14′S.)	
	Juno . . .		14 7	0 29 S
	Venus . .		0 3	1 23 S
17	Sun		1 42	10 33 N
	Mercury .		2 3	12 43 N
	Mars . . .		11 51	3 35 N
	Juno . . .		14 6	0 22 S
	Venus . .		0 8	0 54 S
18	Sun		1 45	10 54 N
	Mercury .		2 11	13 37 N
	Mars . . .		11 50	3 38 N
	Im. * . . .	7.8	13 21 or 11^h 33′ MT.	
	*'s R.A. 18^h 20′ Decl. 25° 0′ N. (5′S.)			
	Juno . . .		14 5	0 15 S
	Em. * . .		14 18 or 12^h 30′ MT. (5′S.)	
	Venus . .		0 12	0 26 S
19	Sun		1 49	11 15 N
	Mercury .		2 19	14 40 N
	Mars . . .		11 50	3 40 N
	Juno . . .		14 5	0 8 S
	Im. * 1 . .	7.8	14 19 or 12^h 27′ MT.	

APRIL.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
	*'s R.A. 19^h 14′ Decl. 22° 54′S. (4′S.)			
	Im. * 2 . .	7.8	14 51 or 12^h 59′ MT.	
	*'s R.A. 19^h 15′ Decl. 22° 47′ S. (1′N.)			
	Em. * 1 .		15 14 or 13^h 22′ MT. (8′S.)	
	Em. * 2 .		15 57 or 14^h 5′ MT. 3′S.)	
	Venus . .		0 17	0 2 N
20	Sun		1 53	11 36 N
	Mercury .		2 27	15 19 N
	Mars . . .		11 49	3 43 N
	Juno . . .		14 4	0 2 S
	Im. * 1 . .	8	14 53 or 12^h 57′ MT.	
	*'s R.A. 20^h 5′ Decl. 19° 41′ S. (5′N.)			
	Em. * 1 .		15 54 or 13^h 58′ MT. (2′S.)	
	Im. * 2 . .	8	16 0 or 14^h 4′ MT.	
	*'s R.A. 20^h 6′ Decl. 19° 26′S. (cont.)			
	Venus . .		0 21	0 30 N
21	Sun		1 57	11 56 N
	Mercury .		2 35	16 8 N
	Mars . . .		11 48	3 45 N
	Juno . . .		14 3	0 4 N
	Venus . .		0 26	0 59 N
22	Sun		2 0	12 16 N
	Mercury .		2 43	16 57 N
	Mars . . .		11 47	3 47 N
	Juno . . .		14 2	0 11 N
	Venus . .		0 30	1 27 N
23	Sun		2 4	12 36 N
	Mercury .		2 51	17 41 N
	Em. 1 Sat.		10 38 or 8^h 31′ MT. (+100)	
	Mars . . .		11 47	3 48 N
	Juno . . .		14 1	0 17 N
	Venus . .		0 34	1 56 N
24	Sun		2 8	12 56 N
	Mercury .		2 59	18 21 N
	Mars . .		11 46	3 49 N
	Juno . . .		14 1	0 23 N
	Venus . .		0 39	2 24 N
25	Sun		2 12	13 16 N
	Mercury .		3 7	19 [illegible] N
	Mars . . .		11 45	3 50 N
	Juno . . .		13 0	0 29 N
	Venus . .		0 43	2 53 N
26	Sun		2 15	13 35 N
	Mercury .		3 11	19 44 N
	Mars . . .		11 45	3 51 N
	Juno . . .		13 59	0 35 N
	Venus . .		0 47	3 21 N
27	Sun		2 19	13 54 N
	Mercury .		3 21	20 20 N
	Mars . . .		11 44	3 51 N
	Juno . . .		13 58	0 41 N
	Venus . .		0 52	3 50 N
28	Sun		2 23	14 13 N
	Mercury .		3 28	20 56 N
	Mars . . .		11 44	3 51 N
	Juno . . .		13 58	0 47 N
	Venus . .		0 56	4 18 N
29	Sun		2 27	14 32 N
	Mercury .		3 36	21 27 N
	Mars . . .		11 44	3 51 N
	Juno . . .		13 57	0 53 N
	Venus . .		1 1	4 46 N
30	Sun		2 31	14 51 N
	Mercury .		3 43	21 57 N
	Im. * 1 . .	7.8	9 59 or 7^h 24′ MT.	
	*'s R.A. 3^h 49′ Decl. 23° 7′ N. (cont.)			
	Im. * 2 . .	7.8	10 22 or 7^h 47′ MT.	
	*'s R.A. 3^h 50′ Decl. 22° 42′ N. (3′ S.)			
	Em. * 2 .		11 8 or 8^h 33′ MT. (4′S.)	
	Mars . . .		11 43	3 50 N
	Em. 1 Sat.		13 1 or 10^h 26′ MT. (+100)	
	Juno . . .		13 56	0 58 N
	Venus . .		1 5	5 15 N

MAY.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
1	Sun		2 34	15 9 N
	Mercury .		3 49	22 23 N
	Mars . . .		11 43	3 49 N
	Im. * . . .	6	13 3 or 10^h 24' MT.	
	*'s R.A. 4^h 57' Decl. 24° 1'N. (1'S.)			
	Em. . . .		13 47 or 11^h 8' MT. (0'.)	
	Venus . .		1 10	5 43 N
2	Sun		2 38	15 27 N
	Mercury .		3 55	22 45 N
	Mars . . .		11 43	3 48 N
	Im. * 1 . .	8	12 8 or 9^h 25' MT.	
	*'s R.A. 5^h 55' Decl. 23° 39' N. (12'S.)			
	Im. * 2 . .		12 23 or 9^h 40' MT.	
	*'s R.A. 5^h 56' Decl. 23° 39' N. (11'S.)			
	Em. * 1 .		12 42 or 9^h 59' MT. (8'S.)	
	Em. * 2 .		13 2 or 10^h 19' MT. (6'S.)	
	Venus . .		1 14	6 10 N
3	Sun		2 42	15 44 N
	Mercury .		4 1	23 6 N
	Mars . . .		11 43	3 46 N
	Im. * . . .	7	13 34 or 10^h 47' MT.	
	*'s R.A. 6^h 0' Decl. 21° 33' N. (9'S.)			
	Em. . . .		14 21 or 11^h 31' MT. (1'S.)	
	Venus . .		1 19	6 38 N
4	Sun		2 46	16 2 N
	Mercury .		4 7	23 28 N
	Im. * 1 . .	7.8	10 35 or 7^h 45' MT.	
	*'s R.A. 7^h 51' Decl. 18° 43' N. (cont.)			
	Im. * 2 . .	7	10 35 or 7^h 45' MT.	
	*'s R.A. 7^h 52' Decl. 18° 43' N. (15'S.)			
	Em. * 2 .		11 19 or 8^h 29' MT. (7'N)	
	Mars . . .		11 42	3 44 N
	Im. * 3 . .	6	15 7 or 12^h 16' MT.	
	*'s R.A. 8^h 2' Decl. 18° 10' N. (9'N.)			
	Em. * 3 .		15 38 or 12^h 47' MT. (16'N.)	
	Venus . .		1 23	7 5 N
5	Sun		2 50	16 19 N
	Mercury .		4 13	23 42 N
	Moon . . .		8 45	14 59 N
	Mars . . .		11 42	3 42 N
	Venus . .		1 28	7 33 N
6	Sun		2 50	16 19 N
	Mercury .		4 13	23 42 N
	Moon . . .		9 41	9 31 N
	Im. * 1 . .	8	11 35 or 8^h 36' MT.	
	*'s R.A. 9^h 45' Decl. 8° 54' N (13'S.)			
	Mars . . .		11 42	3 39 N
	Em. * 1 .		12 41 or 9^h 42' MT. (3'N.)	
	Im. * 2 . .		12 57 or 9^h 58' MT.	
	*'s R.A. 9^h 47' Decl. 8° 30' N. (16'S.)			
	Em. 2 Sat.		13 35 or 10^h 36' MT. (+100.)	
	Em. * 2 .		13 39 or 10^h 40' MT. (15'S.)	

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
	Venus . .		1 32	8 0 N
7	Sun		2 57	16 53 N
	Mercury .		4 24	24 11 N
	Moon . . .		10 36	3 35 N
	Im * . . .	6	10 37 or 7^h 34' MT.	
	*'s R.A. 10^h 36' Decl. 3° 15' N. (7'S.)			
	X. 172 . .	8	10 42	4 31 N
	58 Leonis	5	10 51	4 34 N
	X. 229 . .		10 55	4 35 N
	Mars . . .		11 42	3 37 N
	Em. * . .		11 49 or 8^h 46' MT (11'N.)	
	Venus . .		1 37	8 28 N
8	Sun		3 1	17 9 N
	Mercury .		4 8	24 19 N
	87 Leonis	4.5	11 21	2 2 S
	Moon . . .		11 28	2 36 S
	Mars . . .		11 42	3 34 N
	XI. 179 . .	8	11 45	2 48 S
	XI. 213 . .	7	11 52	0 47 S
	Venus . .		1 42	8 55 N
9	Sun		3 5	17 25 N
	Mercury .		4 33	24 27 N
	Mars . .		11 42	3 30 N
	14 Virg. .	6.7	12 10	7 56 S
	Moon . . .		12 21	8 39 S
	22 Virg. .	5.6	12 25	8 29 S
	31 d. 1. .	6	12 33	8 2 S
	Im. * . . .	7	16 48 or 13^h 37' MT.	
	*'s R.A. 12^h 1' Decl. 2° (11'S.)			
	Em. . .		17 49 or 14^h 37' MT. (1'N.)	
	Venus . .		1 46	9 21 N
10	Sun		3 9	17 41 N
	Mercury .		4 37	24 35 N
	Mars . . .		11 43	3 27 N
	XIII. 19 .	7.8	13 4	12 32 S
	Moon . .		13 15	14 10 S
	75 Virg. .	6	13 23	14 27 S
	XIII. 177 .	7.8	13 35	13 20 S
	Venus . .		1 51	9 48 S
11	Sun		3 13	17 56 N
	Mercury .		4 41	24 37 N
	Mars . . .		11 43	3 23 N
	XIV. 22 .	6	14 6	17 23 S
	Moon . . .		14 11	18 56 S
	XIV. 116 .	7	14 25	19 40 S
	10 Libræ .	7	14 42	17 37 S
	Im. * 1 . .	7.8	16 28 or 13^h 9' MT.	
	*'s R.A. 14^h 16' Decl. 19° 10' S. (12'N.)			
	Im. * 2 . .	7	16 30 or 13^h 11' MT.	
	*'s R.A. 14^h 16' Decl. 19°(10'S.) (12'N.)			
	Em. * 1 .		16 52 or 13^h 33' MT. (16'N.)	
	Em. * 2 .		16 52 or 13^h 33' MT. (15'N.)	

MAY.

Days	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.
	Venus		1 56	10 15 N
12	Sun		3 17	18 12 N
	Mercury		4 45	24 39 N
	Mars		11 43	3 19 N
	XIV. 262	7	14 56	22 38 S
	XIV. 282	6	15 0	23 18 S
	Moon		15 9	22 31 S
	XV. 65		15 16	20 45 S
	Venus		2 0	10 41 N
13	Sun		3 21	18 27 N
	Mercury		4 48	24 41 N
	Mars		11 43	3 15 N
	Im. * 1	7.8	14 16 or 10^{h} 49′ MT.	
	*'s R.A. 16^{h} 6′ Decl. 25° 0′ S. (15′S.)			
	Im. * 2	7	14 20 or 10^{h} 53′ MT.	
	*'s R.A. 16^{h} 4′ Decl. 25° 1′ N. (cont.)			
	Em. * 1		14 37 or 11^{h} 10′ MT. (13′S.)	
	Moon		16 7	24 53 S
	ο Scorpii	5.6	16 10	23 44 S
	5	5	16 15	23 2 S
	22	6	16 20	24 43 S
	Im. *	4	17 29 or 14^{h} 2′ MT.	
	*'s R.A. 16^{h} 10′ Decl. 25° 10′ S. (10′S.)			
	Em. *		18 25 or 14^{h} 58′ MT. (7′S.)	
	Venus		2 5	11 8 N
14	Sun		3 25	18 41 N
	Mercury		4 51	24 38 N
	Mars		11 44	3 10 N
	XVI. 248	6	16 49	24 49 S
	28 Oph.	7	16 53	25 26 S
	Moon		17 6	25 43 S
	θ Oph.	3.4	17 11	24 49 S
	Venus		2 10	11 33 N
15	Sun		3 29	18 55 N
	Mercury		4 54	24 34 N
	Mars		11 44	3 5 N
	Im. * 1	7	14 18 or 10^{h} 44′ MT.	
	*'s R.A. 17^{h} 58′ Decl. 25° 29′ S. (13′S.)			
	Im. * 2	7	14 24 or 10^{h} 50′ MT.	
	*'s R.A. 17^{h} 58′ Decl. 25° 29′ S. (13′S.)			
	Im. * 3	6	14 24 or 10^{h} 50′ MT.	
	Em. * 1		14 56 or 11^{h} 21′ MT. 5 13′s.)	
	*'s R.A. 17^{h} 58′ Decl. 25° 29′ S. (13′S.)			
	Em. * 2 & 3		15 1 or 11^{h} 26′ MT. (13′S.)	
	63 Oph.	6.7	17 41	24 51 S
	4 Sagit.	5	17 49	23 47 S
	XVII. 342	7	17 54	24 24 S
	Moon		18 3	25 10 S
	Venus		2 14	11 58 N
16	Sun		3 33	19 9 N
	Mercury		4 57	24 31 N
	Mars		11 44	3 0 N

Days	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.
	Im. * 1	8.9	13 51 or 10^{h} 16′ MT.	
	*'s R.A. 18^{h} 51′ Decl. 23° 28′ S. (13′N.)			
	Em. * 1		14 29 or 10^{h} 51 MT. (12′N.)	
	Im. * 2		17 51 or 14^{h} 12′ MT.	
	*'s R.A. 18^{h} 58′ Decl. 23° 27′ S. (1′S.)			
	Im. * 3		18 25 or 14^{h} 46′ MT	
	*'s R.A. 18^{h} 59′ Decl. 23° 28′ S. (4′S.)			
	Em. * 2		19 9 or 15^{h} 30′ MT. (1′S.)	
	Em. * 3		19 33 or 15^{h} 54′ MT. (9′S.)	
	Venus		2 19	12 23 N
17	Sun		3 37	19 23 N
	Mercury		4 59	24 22 N
	Mars		11 45	2 55 N
	Im. *	8	18 57 or 15^{h} 14′ MT.	
	*'s R.A. 19^{h} 50′ Decl. 20° 20′ S. (4′S.)			
	Em. *		19 53 or 16 10 MT. (9′S.)	
	Venus		2 24	12 48 N
18	Sun		3 41	19 36 N
	Mars		5 1	24 14 N
	Mercury		11 45	2 49 N
	Venus		2 28	13 13 N
19	Sun		3 45	19 49 N
	Mercury		5 2	24 5 N
	Mars		11 46	2 44 N
	Im. *		17 4 or 13^{h} 13′ MT.	
	*'s R.A. 21^{h} 23′ Decl. 13° 2′ S.) 1′N.)			
	Em. *		18 7 or 14^{h} 16′ MT. (10′S.)	
	Venus		2 33	13 38
20	Sun		3 49	20 2 N
	Mercury		5 2	23 52 N
	Mars		11 46	2 38 N
	Venus		2 38	14 1 N
21	Sun		3 53	20 14 N
	Mercury		5 3	23 40
	Mars		11 47	2 31 N
	Im. *	8.9	17 30 or 13^{h} 31′ MT.	
	*'s R.A. 22^{h} 51′ Decl. 3° 23′ S. (1′N.)			
	Em.		18 28 or 14^{h} 29′ MT. (10′S.)	
	Venus		2 43	14 24 N
22	Sun		3 57	20 26 N
	Mercury		5 3	23 27 N
	Mars		11 47	2 25 N
	Venus		2 47	14 47 N
23	Sun		4 1	20 38 N
	Mercury		5 3	23 11 N
	Mars		11 48	2 18 N
	Venus		2 52	15 11 N
24	Sun		4 5	20 49 N
	Mercury		5 2	22 54 N
	Mars		11 49	2 12 N
	Im. *	7.8	19 24 or 15^{h} 13′ MT.	
	*'s R.A. 1^{h} 9′ Decl. 12° 12′ N. (14′N.)			

MAY.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.	Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.				H. M.	D. M.
	Em. * . .		19 33 or 15^h 22' MT. (13'N.)			Mercury .		4 59	21 42 N
	Venus . .		2 57	15 34 N		Mars . . .		11 52	1 42 N
25	Sun		4 9	21 0 N		Venus. . .		3 17	16 59 N
	Mercury .		5 2	22 38 N	29	Sun		4 25	21 40 N
	Mars . . .		11 50	2 5 N		Mercury .		4 57	21 22 N
	Im. * . . .	7	18 59 or 14^h 45' MT.			Mars . . .		11 53	1 34 N
	*'s R.A. 2^h 0'		Decl. 16° 24' N. (8'N.)			Venus . .		3 22	17 19 N
	Em. * . . .		19 50 or 15^h 36' MT. (2'N.)		30	Sun		4 29	21 49 N
	Venus . .		3 2	15 57 N		Mercury .		4 55	21 2 N
26	Sun		4 13	21 10 N		Mars . . .		11 54	1 27 N
	Mercury .		5 1	22 19 N		Im. * . . .	6.7	11 37 or 10^h 3' MT.	
	Mars . . .		11 50	1 57 N		*'s R.A. 6^h 41'		Decl. 22° 0' N. (6'S.)	
	Venus . .		3 7	16 18 N		Em. * . .		15 20 or 10^h 46' MT. (4'N.)	
27	Sun		4 17	21 20 N		Venus . .		3 27	17 40 N
	Mercury .		5 0	22 0 N	31	Sun		4 33	21 57 N
	Mars . . .		11 51	1 50 N		Mercury .		4 53	20 42 N
	Im. 3 Sat.		13 40 or 9^h 19' MT. (+100)			Mars . . .		11 55	1 18 N
28	Sun		4 21	21 30 N		Venus . .		3 32	18 0 N

JUNE.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.	Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.				H. M.	D. M.
1	Sun		4 37	22 5 N		Mercury .		4 42	19 5 N
	Mercury .		4 51	20 21 N	5	Sun		4 54	22 35 N
	Georgian .		19 6	23 1 S		Moon. . .		12 5	6 55 S
	Venus . .		3 37	18 21 N		Im. * 1 . .	7.8	14 39 or 9^h 42' MT.	
2	Sun. . . .		4 41	22 11 N		*'s R.A. 12^h 8'		Decl. 7° 48' S. (15'S.)	
	Mercury .		4 40	20 1 N		Im. * 2 . .	8	15 13 or 10^h 16' MT.	
	Im. * . . .	7	15 56 or 11^h 11' MT.			*'s R.A. 12^h 9'		Decl. 7° 55'S. (16'S.)	
	*'s R.A. 9^h 36'		Decl. 9° 41' N. (5'N.)			Em. * 1 .		15 15 or 10^h 18' MT. (8'S.)	
	Em		16 35 or 11^h 50' MT. (14'N)			Im. * 3 .		15 31 or 10^h 34' MT.	
	Georgian .		19 6	23 1 S		*'s R.A. 12^h 10'		Decl. 7° 56'S. (14'S.)	
	Venus . .		3 42	18 38 N		Em. * 3 .		15 49 or 10^h 52' MT. (5'S.	
3	Sun. . . .		4 45	22 21 N		Em. * 2 .		15 51 or 10^h 51' MT. (10'S.	
	Moon. . .		10 18	5 15 N		Georgian .		19 5	23 2 S
	Im. * . . .	6.7	15 24 or 10^h 35' MT.			Venus . .		3 57	19 31 N
	*'s R.A. 10^h 27'		Decl. 4° 27' S. (cont.)			Mercury. .		4 40	18 50 N
	Georgian .		19 6	23 1 S	6	Sun. . . .		4 57	22 41 N
	Venus . .		3 47	18 56 N		Moon. . .		12 57	12 32 S
	Mercury .		4 41	19 22 N		Georgian .		19 5	23 2 S
4	Sun. . . .		4 49	22 28 N		Venus . .		4 2	19 48 N
	Moon. . .		11 11	0 53 S		Mercury .		4 39	18 35 N
	Im. * 1 . .	7.8	15 40 or 10 47 MT.		7	Sun. . . .		5 2	22 47 N
	*'s R.A. 11^h 20'		Decl. 2° 1' S. (8'N.)			Moon. . .		13 51	17 28 S
	Im. * 2 . .	4.5	16 11 or 11^h 18' MT.			XIII. 317 .	6	14 1	15 28 S
	*'s R.A. 11^h 21'		Decl. 2° 2' S. (1'S.)			XIV. 22 .	6	14 6	17 23 S
	Em. * 1 .		16 40 or 11^h 47' MT. (7'S.)			XIV. 38 .	7.8	14 10	17 42 S
	Em. * 2 .		17 6 or 12^h 13' MT. (13'S.)			Georgian .		19 5	23 2 S
	Georgian .		19 5	23 2 S		Venus . .		4 7	20 6 N
	Venus . .		3 52	19 13 N		Mercury .		4 37	18 20 N

JUNE.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.
8	Sun		5 6	22 53 N
	Im. * 1 . .		14 13 or 9^h 5′ MT.	
	*'s R.A. 14^h 47′ Decl. 21° 26′ S. (2′S.)			
	XIV. 171 .	7	14 37	20 35 S
	Moon . . .		14 47	21 24 S
	XV. 19 . .	6.7	15 6	21 41 S
	Im. * 2 . .	7.8	15 8 or 9^h 59′ MT.	
	*'s R.A. 14^h 49′ Decl. 21° 41′ S. (10′S.)			
	XV. 65 . .	8	15 16	20 45 S
	Em. * 1 .		15 25 or 10^h 16′ MT. (8′S.)	
	Em. * 2 .		16 14 or 11^h 5′ MT. (6′S.)	
	Georgian .		19 5	23 2
	Venus . .		4 12	20 20 N
	Mercury .		4 35	18 9 N
9	Sun		5 10	22 58 N
	Im. * 1 . .	6	14 29 or 9^h 16′ MT.	
	*'s R.A. 15^h 13′ Decl. 21° 0′ S. (6′N.)			
	Im. * 2 . .		14 45 or 9^h 32′ MT.	
	*'s R.A. 15^h 44′ Decl. 21° 3′ S. (5′N.)			
	Em. * 1 .		15 23 or 10^h 10′ MT. (12′N.)	
	42 Libræ .	5.6	15 30	23 14 S
	XV. 149 .	7.8	15 33	24 51 S
	Moon . .		15 45	24 9 S
	Em. * 2 .		15 47 or 10^h 34′ MT. (11′N.)	
	XV. 225 .	3	15 50	22 7 S.
	Im * 3 . .	6.7	17 4 or 11^h 51′ MT.	
	*'s R.A. 15^h 48′ Decl. 24° 20′ S. (1′S.)			
	Em. * 3 .		18 15 or 13^h 2′ MT. (3′N.)	
	Georgian .		19 4	23 3 S
	Im. * 4 . .	7	19 27 or 14^h 14′ MT.	
	*'s R.A. 15^h 52′ Decl. 24° 31′ S. (4′S.)			
	Em. * 4 .		20 29 or 15^h 15′ MT. (3′N.)	
	Venus . .		4 17	20 35 N
	Mercury .		4 33	17 58 N
10	Sun		5 14	23 3 N
	Im. * 1 . .	6	12 21 or 7^h 8′ MT.	
	*'s R.A. 16^h 36′ Decl. 25° 12′ S. (2′N.)			
	Em. * 1 .		13 23 or 8^h 7′ MT. (6′N.)	
	Im. * 2 . .		15 8 or 9^h 51′ MT.	
	*'s R.A. 16^h 41′ Decl. 25° 18′ S. (7′N.)			
	Im. * 3 . .		15 21 or 10^h 4′ MT.	
	*'s R.A. 16^h 41′ Decl. 25° 17′ S. (8′N.)			
	Em. * 2 .		16 17 or 11^h 0′ MT. (11′N.)	
	Em. * 3 .		16 17 or 11^h 0′ MT. (11′N.)	
	Im. * 4 . .		16 32 or 11^h 15′ MT.	
	*'s R.A. 16^h 44′ Decl. 25° 32′ S. (3′S.)			
	25 Scorpii	6	16 36	25 12 S
	18 Oph.	6	16 39	24 19 S
	Moon . . .		16 43	25 33 S
	26 Oph. .	6	16 49	21 43 S
	Em. * 4 .		17 30 or 12 13 MT. (1′S.)	
	Georgian .		19 4	23 3 S
	Venus . .		4 22	20 49 N
	Mercury . .		4 32	17 47 N
11	Sun		5 18	23 7 N
	Moon . . .		17 40	25 31 S
	63 Oph. .	6.7	17 44	21 51 S
	4 Sagit. . .	5	17 49	23 47 S
	XVII. 312	7	17 54	24 21 S
	Georgian .		19 4	23 3 S
	Venus . .		4 27	21 3 N
	Mercury .		4 31	17 41 N
12	Sun		5 22	23 11 N
	Im. * . . .	8	15 39 or 10^h 14′ MT.	
	*'s R.A. 18^h 32′ Decl. 24° 6′ S. (15′N.)			
	Em. * . .		16 8 or 10^h 43′ MT. (14′N.)	
	XVIII. 129	6	18 28	23 39 S
	XVIII. 141	6	18 31	23 59 S
	Moon . . .		18 37	24 9 S
	ν Sagit. .	5	18 44	22 57 S
	Georgian .		19 4	23 3 S
	Mercury .		4 30	17 36 N
	Venus . .		4 32	21 17 N
13	Sun		5 27	23 14 N
	Georgian .		19 4	23 4 S
	XIX. 138 .	6	19 20	21 40 S
	XIX. 166 .	7	19 25	21 9 S
	Moon . . .		19 30	21 37 S
	56 Sagit. .	6	19 36	20 10 S
	Mercury .		4 29	17 30 N
	Venus . .		4 37	21 32 N
14	Sun		5 31	23 18 N
	Im. * 1 . .	7.8	14 52 or 9^h 20′ MT.	
	*'s R.A. 20^h 11′ Decl. 18° 52′ S. (9′S.)			
	Em. * 1 .		15 48 or 10^h 16′ MT. (2′N.)	
	Im. * 2 . .	8	15 53 or 10^h 21′ MT.	
	*'s R.A. 20^h 13′ Decl. 18° 54′ S. (0′)			
	Em. * 2 .		16 59 or 11^h 26′ MT. (7′S.)	
	Im. * 3 . .	5	19 0 or 13^h 27′ MT.	
	*'s R.A. 20^h 17′ Decl. 18° 46′ S. (cont.)			
	Georgian .		19 4	23 4 S
	Im. * 4 . .	5	19 39 or 14^h 6′ MT.	
	*'s R.A. 20^h 19′ Decl. 18° 23′ S. (3′N.)			
	Im. * 5 . .	7.8	19 43 or 14^h 10′ MT.	
	*'s R.A. 20^h 19′ Decl. 18° 27′ S. (1′S.)			
	XX. 45 . .	8	20 6	16 49 S
	Moon . . .		20 21	
	XX. 194 .	7	20 26	17 7 S
	XX. 240 .		20 31	16 45 S
	Em. * 5 .	6.7	20 53 or 15^h 20′ MT. (11′S.)	
	Em. * 4 .		20 56 or 15^h 23′ MT. (8′S.)	
	Mercury .		4 30	17 30 N
	Venus . .		4 42	21 43 N
15	Sun		5 35	23 20 N

JUNE.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.
	Georgian .		19 4	23 4 S
	Im. * 7 . .	7	19 51 or 14^{h} 14′ MT.	
	*'s R.A. 21^{h} 8′ Decl. 14° 0′ S. (12′N.)			
	Em. * . .		21 1 or 15^{h} 24′ MT. (0′)	
	Mercury .		4 31	17 30 N
	Venus . .		4 47	21 54 N
16	Sun		5 39	23 23 N
	Im. * 1 . .	7.8	16 55 or 11^{h} 14′ MT.	
	*'s R.A. 21^{h} 48′ Decl. 10° 24′ S. (11′S.)			
	Em. * 1 .		17 14 or 11^{h} 33 MT. (15′S.)	
	Georgian .		19 3	23 4 S
	Im. * 2 . .	7.8	20 44 or 15^{h} 3′ MT.	
	*'s R.A. 21^{h} 54′ Decl. 9° 21′ S. (13′N.)			
	Em. * 2 .		21 48 or 16^{h} 6′ MT. (1′N.)	
	Mercury .		4 31	17 30 N
	Venus . .		4 52	22 5 N
17	Sun		5 43	23 24 N
	Georgian .		19 3	23 5 S
	Mercury .		4 32	17 36 N
	Venus . .		4 57	22 15 N
18	Sun		5 47	23 26 N
	Im. * . . .	6	18 58 or 13^{h} 9′ MT.	
	*'s R.A. 23^{h} 18′ Decl. 0° 10′ N. (7′S.)			
	Georgian .		19 3	23 5 S
	Em. * . . .		19 55 or 14^{h} 6′ MT. (0′.)	
	Mercury .		4 32	17 41 N
	Venus . .		5 2	22 26 N
19	Sun		5 52	23 27 N
	Georgian .		19 3	23 5 S
	Mercury .		4 33	17 47 N
	Venus . .		5 8	22 37 N
20	Sun		5 56	23 28 N
	Georgian .		19 3	23 6 S
	Mercury .		4 35	17 57 N
	Venus . .		5 13	22 44 N
21	Sun		6 0	23 28 N
	Georgian .		19 3	23 6 S
	Mercury .		4 38	18 8 N
	Venus . .		5 19	22 51 N
22	Sun . * . .		6 4	23 28 N
	Georgian .		19 3	23 6 S
	Mercury .		4 40	18 18 N
	Venus . .		5 24	22 58 N
23	Sun		6 8	23 27 N
	Georgian .		19 3	23 7 S
	Im * . . .	6.7	20 13 or 14^{h} 5′ MT.	
	*'s R.A. 8^{h} 28′ Decl. 22° 5′ N. (13′S.)			
	Em. * . . .		20 47 or 14^{h} 38′ MT. (7′S.)	
	Mercury .		4 43	18 32 N
	Venus . .		5 29	23 4 N
24	Sun		6 12	23 26 N
	Georgian .		19 2	23 7 S
	Mercury .		4 45	18 46 N
	Venus . .		5 35	23 11 N
25	Sun		6 16	23 25 N
	Georgian .		19 2	23 7 S
	Mercury .		4 48	19 0 N
	Venus . .		5 40	23 18 N
26	Sun		6 21	23 23 N
	Georgian .		19 2	23 7 S
	Mercury .		4 52	19 16 N
	Venus . .		5 45	23 21 N
27	Sun		6 25	23 20 N
	Georgian .		19 2	23 8 S
	Mercury .		4 56	19 32 N
	Venus . .		5 51	23 25 N
28	Sun		6 29	23 18 N
	Georgian .		19 2	23 8 S
	Mercury .		5 0	19 48 N
	Venus . .		5 56	23 28 N
29	Sun		6 33	23 15 N
	Georgian .		19 1	23 8 S
	Mercury .		5 4	20 6 N
	Venus . .		6 1	23 32 N
30	Sun		6 37	23 11 N
	Georgian .		19 1	23 9 S
	Mercury .		5 9	20 24 N
	Venus . .		6 7	23 36 N

Art. XI. ASTRONOMICAL AND NAUTICAL COLLECTIONS.

No. XVII.

i. *Remarks on the* Catalogue *of the Orbits of the* Comets *that have been hitherto computed.* By Dr. Olbers.

The Catalogue of the Orbits of Comets is founded on that which Delambre has given in the third volume of his Astronomy, p. 409. Many errors of the pen and of the press, in Delambre's Catalogue, are corrected, and those orbits are added which were unknown to Delambre, or overlooked by him, or which have been computed since the termination of his catalogue in 1813. Where several persons have computed the orbits of the same comets, some of their results have been omitted, when they have been manifestly incorrect, or derived only from a construction, or given merely as examples of computation with inadequate observations, and by no means intended to represent the correct orbits. Perhaps, however, too many inaccurate computations have still been retained: but this has been done with the intention of affording a conjecture how far the orbit may be more or less remote from a parabola: and where the orbit has been found decidedly elliptical, it is interesting to compare the difference of the parabolic and the elliptic elements. And since so many orbits have now been computed as elliptic or hyperbolic, a separate column has been added for the eccentricities. Where this is left blank, the eccentricity is supposed to be = 1, or the orbit to be parabolic. The eccentricity shows whether the orbit is elliptic or hyperbolic, and thus renders the elements complete, since the greater axis is easily found from the eccentricity and the least distance. The logarithm of the mean motion is assigned in all cases, on account of its utility in computing the true anomaly, even in the cases of elliptic and hyperbolic orbits. For this logarithm of the mean motion we have retained, on account of uniformity, the constant logarithm 9.9601283, which has hitherto been commonly used, as the logarithm of the mean motion of a comet, of which the least distance is = 1. This value supposes properly that the mass of the comet is equal to that of the earth: but if this mass, which is indeed unknown, but which is certainly always very inconsiderable became = 0, the logarithm should be 9.9601277: so that if we required the greatest possible accuracy, it would be necessary to diminish the tabular logarithm of the mean motion by 6 in the 7th place of decimals.

With respect to the following remarks on the table of comets, I must gratefully acknowledge the assistance that I have received from the excellent notes which the Baron von Zach and the Baron von Lindenau have respectively added to their tables. But for the sake of brevity, I have omitted many references which may be found in Pingré, or in other works here quoted, and very extensively circulated.

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1. 210. Chinese observations. A very uncertain orbit. Mon. Corr. X. p. 167.
2. 539. From Chinese observations, without any latitudes. Mém. prés. à l'Inst. I. p. 290. Mon. Corr. II. p. 415. XVI. p. 498.
3. 565. Deduced from two Chinese observations only, upon the two suppositions, that the curtate distance of the comet, at the time of the first observation, was either = 1.2 or = 1.3. Al-

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though the elements have some resemblance with those of the comets of 1683 and 1739, yet Burckhardt found that neither of these two orbits would accord with the observations of 565. Mon. Corr. X. p. 162.

4. 837. Chinese observations. Pingré Com. I. p. 340.

5. 989. Chinese observations. A very uncertain orbit. Mon. Corr. X. p. 167.

6. 1066. Very uncertain orbit. Pingré I. p. 373.

7. 1097. From Chinese observations of the 6th, 16th, and 17th October. Sav. Etr. I. p. 290. Mon. Corr. II. p. 417. XVI. p. 501.

8. 1231. Chinese observations. Pingré I. p. 401.

9. 1264. Phil. tr. XLVII. p. 281. Pingré I. p. 406. Mém. Par. 1760. p. 195. Struyck Vervolg Amst. 1753. p. 108, 109. The identity with the comet of 1556 is uncertain from the want of precision in both orbits.

10. 1299. Two European observations, and one Chinese: a third European record does not agree. Pingré I. p. 418.

11. 1301. Pingré has applied a correction to the European, and Burckhardt to the Chinese observations, which could not otherwise be reconciled. Hence the diversity. Pingré I. p. 420. Mon. Corr. X. p. 164.

12. 1337. Pingré I. p. 432. The orbit of Pingré is preferable to that of Halley, since it represents both the European and the Chinese observations tolerably well, while Halley's differs as far as 20° from the latter.

13. 1351. Even the few elements, which Burckhardt has been able to assign, are very uncertain. Pingré I. p. 437. Mon. Cor. II. p. 415. Mem. Sav. Etr. I. p. 290. There are only four Chinese observations, of the 24th, 26th, 29th, and 30th November, without latitudes. On the whole we can place no manner of reliance on the orbits of the comets of 240, 539, 565, 989, 1066, 1097, 1231, 1299, 1301, 1351, and 1362.

14. 1362. Mon. Corr. X. p. 166. Three Chinese observations. The two orbits are derived from different suppositions respecting the latitudes.

15. 1456. The celebrated comet of Halley, of which the period amounts to about 76 years. Pingré I. p. 459.

16. 1472. From the observations of Regiomontanus. Pingré I. p. 475.

(15.) 1531. Halley's comet as observed by Apian. Pingré I. p. 488. See also especially Halley's Tabulæ Astronomicæ, and his essay there inserted, De motu cometarum elliptico.

17. 1532. Pingré I. p. 492. The once supposed identity of this comet with that of 1661 must be abandoned. Méchain, Mém. Prés. X. p. 333. Olbers in Hindenburg's Magazine for Mathematics 1787. p. 440.

18. 1533. Pingre I. p. 496. The total diversity of the two orbits sufficiently shows the uncertainty of both. Struyck, 1753. p. 24. Astron. Jahrb. Berl. 1800.

(9.) 1556. From the observations of Paul Fabricius between the 4th and the 17th of March, which cannot be considered as certain; so that we can place little reliance on the resemblance to the still less certain elements of the comet of 1264. Pingré I. p. 502.

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19. 1558. From three observations of the Landgrave of Hesse, and one of Cornelius Gemma, the latter being corrected in what is, very probably, an error of the press. Gemma de Naturæ Divinis Characterismis. Book II. ch. i. p. 33. See Berl. Astr. Jahrb. 1817, p. 176.
20. 1577. From Tycho's Observations. Pingré I. p. 511.
21. 1580. Halley from Möstlin, Pingré from Tycho's better observations. Pingré I. p. 521.
22. 1582. Both orbits uncertain, since they are founded only on three observations of Tycho, of the 13th, 17th, and 18th of May; that of the 18th giving a double result, whence the two orbits are derived. The first elements seem the most probable, Pingré I. p. 544.
23. 1585. From the observations of Tycho and Rothmann. Pingré I. p. 550.
24. 1590. Tycho's observations, from 23 February to 6 March. Pingré I. p. 554.
25. 1593. According to the observations of Chr. J. Ripensis, at Zerbst. Mém. Par. 1747, p. 562. Pingré I. p. 557.
26. 1596. Halley from Möstlin, Pingré from Tycho's observations: hence the latter elements are preferable. Pingré I. p. 562.
(15) 1607. Halley's comet. Pingré II. p. 3. Halley's Tab. Astr. First supplement of the Berl. Astr. Jahrb. Mon. Corr. X. p. 425.
27. 1618. From Kepler's imperfect observations. Kepler de Cometis. Pingré II. p. 4.
28. 1618. Bessel's orbit is far the best, being founded on the observations of Harriot, Longomontanus, Cysat, and Schnellius. Berl. Astr. Jahrb. 1808, p. 113.
29. 1652. From the observations of Hevelius between the 20th December and the 8th January. Hevelius's observations are not only in the second volume of the Machina Cœlestis, which is very rare, but also in his Cometographia.
30. 1661. The observations of Hevelius from the 3d February to the 28th March. Machina Cœlestis II., and Cometographia. Mém. prés X. p. 350.
31. 1664. Hevelius's observations in the Prodromus Cometicus, or better in the Mantissa Prodromi, and in the Machina Cœl. II. p. 439. Pingré II. p. 10.
32. 1665. From Hevelius's observations from the 6th to the 20th April, which are found in the Descr. Comet. 1665, Mantissa Prodr. Com. and Mach. Cœlestis II.
33. 1672. According to Hevelius's observations from the 6th March to the 21st April. Mach. Cœl. II. p. 593.
34. 1677. According to Hevelius's observations from the 29th April to the 8th May. Flamstead observed it also twice. Mach. Cœl. II. p. 292. Flamstead Hist. Cœl. Br. Ed. 1712, p. 103. Ed. 1725. I. p. 103.
35. 1678. From Lahire's observations, which are only estimated, and from the chart in the Hist. Cél. of Lemonnier, p. 238. See particularly Struyck, 1753, p. 38, 39.
36. 1680. Euler's elliptical elements are to be considered merely as an example of calculation, and require no further consideration. It is only the elliptical orbit of Encke that is of any value at present; it is taken from his masterly prize essay on this comet

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(Zeitschrift für Astr. 1818), in which all the observations are collected and discussed. The first orbit of Encke is the parabola which agrees best with the observations. The longitudes are reckoned from the mean equinox of the 18th December, 1680.

(15.) 1682. Halley's comet: observed from 25th August to 19th September. Flamst. H. C. Br. I. p. 108. Hevel. Ann. Climact. p. 120. Halley in Tab. Astr. de mot. com. ellipt.

37. 1683. According to Flamstead's observations, from the 23d July to the 5th Sept. Flamst. p. 110.

38. 1684. According to Bianchini's observations, from the 1st to the 17th July. Phil. Tr. N. 169, p. 920. Acta erud. 1685. p.241.

39. 1686. Seen first in August in the East Indies, then in September, in Europe. Orbit not very certain. Pingré II. p. 28.

40. 1689. Very uncertain observations, Pingré II. p. 29.

41. 1695. Burckhardt computed his orbit from manuscript observations left by Delisle in the Dépôt de la Marine. What was before known of this comet Pingré has collected. p. 33. Conn. des tems, 1817, p. 278.

42. 1698. The observations of Lahire and Cassini, the only ones that we have of this comet, are deficient in accuracy. Anc. Mém. II. p. 341. X. p. 742. Mém. 1701, p. 117.

43. 1699. Observed by Fontenay at Pekin, and by Cassini and Maraldi at Paris. The observations extend from the 17th February to the 2d March. Mém. Par. 1701, p. 47.

44. 1701. From observations made by P. Pallu at Pau, which had lately been recovered, and from the observations of P. Thomas, at Pekin. Conn. des tems, 1811, p. 482. Noel Obs. Phys. Math. in India fact. p. 128.

45. 1702. The observations between the 20th April and the 5th May not very exact. Struyck, 1753, p. 50. Pingré II. p. 38. Mém. Inst. II. p. 28. Mon. Cor. XVI. p. 511.

46. 1706. Cassini and Maraldi, from the 18th March to the 16th April. Mém. Par. 1706, p. 91, 148. Pingré II. 39. Struyck, 1753, p. 54.

47. 1707. The observations extend from the 25th November to the 23d January, 1708. Mém. Par. 1707, p. 588, and 1708, p. 89, 323. On the orbits see Pingré II. p. 40. Struyck, 1753, p. 54. The orbit of Hottuyn, given imperfectly by Struyck himself, depends only on a construction.

48. 1718. From Kirch's observations, which are not particularly accurate. Misc. Ber. III. p. 200. Phil. Trans. XXX. XXXII. Pingré II. p. 41. Struyck Inleiding de Algemeene Geographie, p. 295. Struyck, 1753, p. 57.

49. 1723. Was seen in the East Indies as early as the 12th October. The orbits are principally founded on the observations made between the 20th October and the 18th December by Halley, Bradley, Pound, and Graham. Phil. Trans. XXXIII. n. 382, p. 41. n. 397, p. 223. The second orbit, ascribed to Struyck, is only found in the astronomical tables of Berlin. Pingré II. p. 42. Burckhardt's hyperbolic orbit. Conn. des tems, 1821.

50. 1729. Discovered by Father Sarabat the 31st July, 1729, and observed until the 18th Jan. 1730. Pingré II. p. 42. Struyck, 1740, p.

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297, 1753, p. 58. Mém. Par. 1730, p. 284. The hyperbolic and parabolic orbits of Burckhardt. Conn. des tems, 1821.

51. 1737. Computed from Bradley's own observations, extending from 26 Feb. to 2 April. Phil. Trans. N. 446, p. 111. Pingré II. p. 45. Struyck, 1740, p. 301.

52. 1737. The observations made at Pekin were published in the Mon. Corr. XXI. p. 316. Conn. des tems. 1812, p. 409.

53. 1739. The observations are by Zanotti, from the 28th May to the 18th August. N. Acta Erudit. 1740, p. 166. Comm. Inst. Bon. II. p. iii, p. 73. Struyck, 1753, p. 64. The second orbit, by Zanotti, is only á first approximation, still remaining imperfect.

54. 1742. For the numerous observations of this comet see Pingré II. 47.

55. 1743. In part very imperfectly observed. The observations are principally collected in Struyck, 1753, p. 75.

56. 1743. Observed imperfectly, and by Klinkenberg alone, from 18 Aug. to 13 Sept. Struyck, 1753, p. 76, 77. The observations, which are also inserted by Pingré II, p. 52, differ sometimes 1° and more from the elements assigned.

57. 1744. Besides the observers and computers of this celebrated comet quoted by Pingré, II. p. 52, and Struyck, p. 78, some valuable matter may be found in Chéseaux Traité de la Comète, Laus. 1744, and Hiorter Trans. Swed. Acad. of Sciences.

58. 1747. Discovered by Chéseaux the 13th Aug. 1746, and last observed by Maraldi the 5th Dec. 1746. The orbits of Maraldi and Lacaille are the best. Pingré II. p. 57. Struyck, 1753. p. 92.

59. 1748. Especially observed by Maraldi, Mém. Par. 1748, p. 229.

60. 1748. Observed only three times imperfectly by Klinkenberg, the 19th, 20th, and 22d May. Struyck, 1753, p. 96. Bessel has reduced the observations with greater care. Berl. Astr. Jahrb. 1809, p. 99. The imperfect elements, time of the Perihelium, 1748, 22d April, ☊ 9ˢ, 24°, Inclination 76°, Least distance 0.5, Motion retrograde, which Delambre ascribes to Burckhardt, and respecting which I can find no further information, cannot possibly belong to either of the computed comets of this year: so that they must have been derived from the alleged observations of the silly Kinderman, which deserve no credit whatever.

61. 1757. Bradley's observations and elements are preferable to the rest. Phil. tr. L. Part. i. p. 408. The other observations may be found collected by Pingré, Mém. Par. 1757, p. 97.

62. 1758. Messier observed the comet from the 15th Aug. to the 2d Nov. Mém. Par. 1759, p. 154, 1760, p. 165, 463.

(15). 1759. Celebrated and predicted re-appearance of Halley's comet. Pingré II. p. 63, gives references to works in which the observations and the elements may be found. Burckhardt's orbit, preferable to all the rest. Conn. des tems, 1819.

63. 1759. Pingré prefers his own elements. The comet was observed from the 25th June to the 16th March 1760. Pingré II. p. 68.

64. 1759. Observed from the 8th Jan. to the 8th Feb. 1760. Pingré II. p. 70.

65. 1762. Discovered by Klinkenberg the 17th May, observed to the 2d July. The refraction had been neglected in the reduction of

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the observations, and hence all the computed orbits varied several minutes from the observations. Burckhardt reduced them with greater care, and hence obtained the last elements, which are more correct. Mém. Inst. VII. p. 226. Mon. Corr. XVI. p. 515.

66. 1763. Discovered by Messier the 28th Sept., observed before the perihelium from the 30th Sept. to the 25th Oct., after the perihelium from the 12th to the 25th Nov. Pingré and Lexell could not represent the observations sufficiently well by any conic section. Pingré II. p. 103. Acta. Ac. Sc. Petr. 1780. Pt. ii. p. 324. Burckhardt has corrected the observations of Messier, which were distorted by some errors in the places of Flamstead's stars, and he has employed the observations of St. Jaques de Silvabelle, which were made public more lately. Mon. corr. X. p. 507. Conn. des tems. XIII. p. 344.

67. 1764. Discovered by Messier, and observed from 3d Jan. to 11th Feb. The third orbit is that which has been corrected from all the observations. Pingré II. p. 74.

68. 1766. Discovered by Messier the 8th March, and observed for eight days only. Pingré II. p. 75.

69. 1766. Observed by Messier at Paris only five days, from the 8th to the 12th April. La Nux at the Isle of Bourbon followed it from the 29th April to the 13th May. Pingré II. p. 76. *The imperfect observations of La Nux, Pingré could not* satisfactorily combine with the Parisian observations, and Burckhardt has attempted to do this by means of an ellipsis. Conn. des tems. 1821.

70. 1769. Discovered by Messier the 8th Aug., and observed before the perihelium to the 15th Sept.; after it from the 24th Oct. to the 1st Dec. The principal observations are found Mém. Par. 1769, p. 49; 1770, p. 24; 1775, p. 392. Maskelyne Astr. Obs. I. On the various orbits, besides Pingré II. p. 83, see especially Euler Recherches et Calculs sur la comète, 1769; 4 Pet. 1770; the two rare works of Asclepi De cometarum motu exercitatio; 4 Rom. 1770, and De cometarum motu Addenda, Rom. 1770; besides Bessel's excellent prize memoir in the Berl. Astr. Jahrb. 1811.* That Bessel's elliptic orbit is preferable to all others, scarcely requires to be observed. The nodes and perihelium are determined by Bessel as at rest with respect to the stars for the 1st Jan. 1769.

71. 1770. This comet, celebrated for an orbit deviating so greatly from the parabolic form, was discovered the 14th June by Messier, and observed till the 2d Oct. The observations have been collected by Messier, Mém. Par. 1776, p. 597. The short period of this comet, little exceeding five years and a half, which appeared so paradoxical when it was first computed by Lexell, was fully confirmed by Burckhardt's investigations in his valuable prize memoir. Mém. Inst. 1806, p. 1. The reason why the comet has not re-appeared since 1770, has been very satisfactorily explained in Laplace's Méc. Cel. vol. IV.

72. 1770. Was observed only four times at Paris, between the 10th and 20th of Jan. 1771. Mém. Par. 1771. Pingré II. p. 90.

73. 1771. Discovered by Messier the 1st April, and observed by him until the 19th June; but by St. Jaques de Silvabelle at Mar-

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seilles till the 17th July. The orbit appears, according to the investigations of Burckhardt and of Encke, to be truly hyperbolical. The parabola of Encke, however, affords also results varying but little from the truth. Mon. Corr. X. p. 510. Conn. des tems XIII. p. 344. Von Zach Corresp. astr. 1820. Encke reckons from the mean equinox, 1st Jan. 1771.

74. 1772. Discovered by Montaigne the 8th March, observed by Messier four times only, the 26th, 27th, and 30th March, and the 1st April. The computation of the elliptic orbits was undertaken on account of the similarity of the elements of the second comet of 1805. These more correct investigations render the identity of the two comets highly improbable, and it is accordingly rejected by Bessel, as well as by Burckhardt, who was able to employ the rediscovered observations of Montaigne. Mon. Corr. XIV. p. 73, 84. Conn. des tems 1811, p. 486.

75. 1773. Discovered by Messier the 12th Oct. 1773, and observed until the 14th Apr. 1774. Burckhardt, who employed also observations of St. Jaques de Silvabelle, found the orbit not sensibly different from a parabola. Mém. Par. 1774—1777. Acta Petr. 1779, p. 335. Conn. des tems XIII. p. 343. Mon. Corr. X. p. 540.

76. 1774. Discovered by Montaigne the 11th Aug. observed till the 25th Oct. Mém. Par. 1775, p. 445. Conn. des tems 1821.

77. 1779. Discovered by Bode the 6th Jan. observed till the 17th May. Mém. Par. 1779, p. 318. Pingré, II. p. 94, seems to prefer the orbit of Dangos before the rest.

78. 1780. Discovered by Messier the 27th Oct. and observed till the 29th Nov. Mém. Par. 1780, p. 515. Act. Petr. 1780, p. ii. p. 347. Berl. Astr. Jahrb, 1784, p. 141.

79. 1780. Discovered the 18th Oct. by Montaigne and Olbers, and observed very imperfectly three times only. The elements are therefore very uncertain. Mém. Par. 1780, p. 515. Berl. Astr. Jahrb. 1804, p. 172.

80. 1781. Discovered by Méchain the 28th June, observed till the 15th July. Mém. Par. 1781, p. 349; 1782, p. 581. Berl. Astr. Jahrb. 1785, p. 166.

81. 1781. Discovered by Méchain the 9th Oct. observed till the 25th Dec. Mém. Par. 1781, p. 360; 1782, p. 587. Legendre Nouv. Méthode, p. 41.

82. 1783. Discovered by Pigott the 20th Nov. observed till the 21st Dec. Mém. Par. 1783, p. 123, 643, Phil. tr. LXXIV. Conn. des tems 1788. But respecting Burckhardt's orbit, see especially Conn. des tems 1820, p. 305.

83. 1784. Seen by De la Nux at the Isle of Bourbon the 15th Dec. 1783, in Paris not till the 24th January, and observed there till the 11th March. Afterwards it was again visible, and observed from the 9th to the 26th of May. Mém. Par. 1784, p. 313, 358. The first elements are the most correct.

The comet hitherto inserted in the tables, as the second of 1784, is a shameful forgery of the Chevalier Dangos, as Professor Encke has shown in the Corresp. Ast. for 1820. Cah. v. p. 456.

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84. 1785. Discovered by Messier and Méchain the 7th Jan. observed till the 8th Feb. Mém. Par. 1785, p. 646. Berl. Astr. Jahrb. 1789, p. 142. Conn. des tems, 1788.

85. 1785. Discovered by Méchain the 11th March, observed till the 17th Apr. Mém. Par. 1785, p. 646. Berl. Astr. Jahrb. 1789, p. 143.

86. 1786. Was discovered by Méchain the 17th Jan. and could only be observed once more on the 19th, by Méchain and Messier. Mém. Par. 1786, p. 95. But since the identity of this comet with those of the years 1795, 1805, and 1819, has been demonstrated, Encke was able to determine the orbit from these two observations. Berl. Astr. Jahrb. 1822. Corresp. Astr. 1819. Conn. des tems, 1819, p. 224.

87. 1786. Discovered the 1st Aug. by Miss Caroline Herschel, observed till the 26th Oct. Mem. Par. 1786, p. 98. Maskelyne, Astr. Obs. II. p. 29. Ephem. Milan. 1789. Conn. des tems, 1789.

88. 1787. Discovered by Méchain the 10th April, observed at Paris till the 20th; at Marseilles till the 26th May. Mém. Par. 1787, p. 70. Conn. des tems, 1790. Berl. Astr. Jahrb. 1791. La Nux observed it at the Isle of France from the 25th May to the 21st June.

89. 1788. Discovered by Messier the 25th Nov. observed till the 29th Dec. Mem. Par. 1789, p. 663. Conn. des tems. 1791. Berl. Astr. Jahrb. 1793.

90. 1788. Discovered by Miss Herschel the 21st Dec.: observed last by Méchain the 18th Jan. 1789. Phil. tr. LXXVII. p. 1. Mém. Par. 1789, p. 681. Maskelyne Astron. Obs. for 1788. Berl. Astr. Jahrb. 1793. p. 119.

91. 1790. Discovered by Miss Herschel the 7th Jan.: observed only four times; the 9th, 19th, 20th, and 21st Jan. Mém. Par. 1790, p. 309. The first orbit agrees best with the longitudes; the second with the latitudes.

92. 1790. Discovered by Méchain the 9th of Jan. and observed till the 22d. Mém. Par. 1790, p. 313. Conn. des tems, 1792, p. 355. Berl. Astr. Jahrb. 1794.

93. 1790. Discovered by Miss Herschel the 17th April, and observed till the 29th June. Mém. Par. 1790, p. 320. Conn. des tems 1792, p. 355. Englefield on Comets, Lond. 1793, p. viii.

94. 1792. Discovered by Miss Herschel the 15th of December 1791: observed last by Maskelyne on the 25th Jan. 1792. Berl. Astr. Jahrb. 1795, p. 181, 201; 1796, p. 148; Conn. des tems 1793, p. 374. Englefield on Comets. The first elements, by Méchain, are those which have been corrected from all the observations.

95. 1792. Discovered by Méchain the 10th Jan. 1793; also by Piazzi; observed till the 19th Feb. Piazzi della spec. astr. book v. Berl. Astr. Jahrb. 1797, p. 136; 1799, p. 192. Conn. des tems, 1795, p. 286.

96. 1793. Discovered by Messier the 27th Sept. observed till the 11th Oct.: then seen again the 30th Dec. and observed till the 4th Jan. 1794. Conn. des tems, 1795, p. 287.

(To the best of my knowledge, Messier's observations of this comet, as well as of some others, have not yet been printed. Their entire publication would be highly desirable.)

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97. 1793. Discovered by Perny the 24th September, observed till the 3d December. Conn. des tems, 1795, and especially Conn. des tems, 1820. Burckhardt leaves it doubtful whether this comet is or is not identical with that of 1783.

(86.) 1795. Discovered by Miss Herschel, the 7th November, and observed till the 27th. The observations chiefly rather inaccurate. This is the second appearance of Encke's comet. Phil. Trans. 1795. Berl. Astr. Jahrb. 1799. 1814. More especially see Berl. Astr. Jahrb, 1822, p. 183. Von Zach Corr. Astr. 1819. Encke computes from the mean equinox of the 18th November, 1795.

98. 1796. Discovered by Olbers the 31st March, and observed till the 14th April. Berl. Astr. Jahrb. 1799, p. 106.

99. 1797. Discovered by Bouvard the 14th August, and observed till the 31st. Berl. Astr. Jahrb. 1800. Allg. Geogr. Ephem. I. p. 127, 366, 604.

100. 1798. Discovered by Messier, the 12th April, and observed till the 24th May. Mém. Inst. II. p. 430. Allg. G. Eph. I. p. 679, 692, 694. II. p. 79, 95. Berl. Astr. Jahrb. 1801, p. 231.

101. 1798. Discovered the 6th December by Bouvard, and the 8th by Olbers, and only observed till the 12th. Berl. Astr. Jahrb, 1802, p. 195. Allg. G. Eph. III. Conn. des tems, 1804, p. 373.

102. 1799. Discovered the 6th of August, by Méchain, and observed till the 21st October. Allg. G. Eph. IV. Berl. Astr. Jahrb. 1802, 1803. Mon. Corr. I. II.

103. 1799. Discovered the 26th Dec. by Méchain, and observed by him till the 5th Jan. 1800. Mon. Corr. I. p. 191, 299, Mém. Inst. II. p. 153. Berl. Astr. Jahrb. 1803. Conn. des tems, 1804, p. 376. Méchain thinks that this comet may possibly have been identical with that of 1699.

104. 1801. Discovered almost the same hour by Pons, at Marseilles, and Messier, Méchain, and Bouvard, at Paris, on the 12th July: observed last by Méchain, the 23d. Mon. Corr. IV. p. 179. Berl. Astr. Jahob. 1805, p. 129. Conn. des tems, An. XIII. p. 344, 484.

105. 1802. Discovered by Pons the 26th August: observed till the 3d October. Mon. Corr. VI. Conn. des tems. An XIII, p. 236, 374. Berl. Astr. Jahrb. 1805, 1806, p. 129.

106. 1804. Discovered by Pons the 7th March: observed till the 1st April. Conn. des tems, XV. p. 374: 1808, p. 336. Mon. Corr. IX. Berl. Astr. Jahrb. 1807.

(86.) 1805. Third appearance of the comet of Encke. Discovered at the same time by Bouvard, Pons, and Huth, the 20th October: observed till the 15th, and seen on the 19th November. Mon. Corr. XIII, XIV. Conn. des tems, 1808, 1809. Berl. Astr. Jahrb. 1809. But especially see Berl. Astr. Jahrb. 1822, 1823. Corresp. Astr. 1819.

107. 1805. Discovered by Pons the 10th Nov. and observed till the 9th Dec. Its supposed identity with the comet of 1772 has given occasion to many computations. This identity has not been confirmed; but Gauss found that the observations agree better with any ellipsis that has its greater semiaxis longer than 2.82, than with a parabola. Mon. Corr. XIII. XIV. XXVI.

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p. 360, 490. Berl. Astr. Jahrb. 1809. Conn. des tems. 1808, 1809.

108. 1806. Discovered by Pons the 10th November: observed first till the 20th Dec., and then again from the 17th Jan. to the 12th Feb. 1807. Mon. Corr. XV. XVI. Berl. Astr. Jahrb. 1810. Conn. des tems, 1810. p. 298, 1819.

109. 1807. Great comet. Observed from the 22d Sept. 1807 to the 27th March, 1808. Most of the observations are collected in Bessel's Untersuchungen über den grossen cometen, 4. Königsberg, 1810. Besides this classical work see also Mon. Corr. XVI. XVII. XVIII. XIX. Berl. Astr. Jahrb. 1811, 1812, 1813, 1814. Cacciatore della cometa di 1807. Conn. des Tems, 1809, 1810, 1811, and Phil. Trans. 1808.

110. 1808. Discovered by Pons, the 24th June, and somewhat uncertain, especially with respect to the declinations: observed only at Marseilles, from the 26th June till the 3d July. Mon. Corr. XVIII. p. 245, 359. Berl. Astr. Jahob. 1812, p. 129.

111. 1810. Discovered the 22d August by Pons, and observed very doubtfully at Marseilles only from the 29th Aug. to the 21st Sept. Mon. Corr. XXIII. p. 302., XXIV. p. 71. Berl. Astr. Jahrb, 1814, p. 179.

112. 1811. Discovered by Flauguergues the 26th March, and observed in Europe before the perihelium till the 2d June, after the perihelium from the 20th August to the 11th Jan. 1812: lastly rediscovered by Wisniewski, at New Tsherkask, the 31st July, 1812, and observed again from the 8th to the 17th of August. Upon this great comet, very remarkable even in its form, and observed by almost all astronomers, besides the Mon. Corr. XXIII. XXIV. XXV. Phil. Trans. 1812. Berl. Astr. Jahrb. 1815, 1816. Conn. des tems, 1820, and so forth, see especially the excellent treatise of Dr. F. W. A. Argelander, Ueber die Bahn des grossen cometen von 1811, 4, Konigsberg, 1822. The orbit of Argelander, inserted in the table, is that which he considers as the most probable: but the observations made at the different times of the comet's appearance could not be quite satisfactorily represented by any orbit governed by the Keplerian laws. Argelander reckons from the place of the mean equinox the 12th Sept. 1811.

113. 1811. Discovered by Pons the 16th November, and observed last at Bremen, the 16th February, 1812. Conn. des tems, 1820. Mon. Corr. XXIV, XXV, and especially XXVII. Nicolai reckons from the mean equinox on the 1st Jan. 1812.

114. 1812. Discovered by Pons the 20th July, and observed till the end of September. Mon. Corr. XXVII. XXVIII. Con. des tems, 1820: but see especially the Zeitschrift for 1817. Encke reckons the longitudes from the mean equinox of the 1st September, 1812.

115. 1813. Discovered by Pons the 4th February, and observed till the 11th March. Mon. Corr. XXVII. Conn. des tems, 1820.

116. 1813. Discovered by Pons at Marseilles, and Harding, at Gottingen, the 2d and 3d of April: observed till the 29th. Conn. des tems, 1820. Mon. Corr. XXVII. XXVIII.

117. 1815. Discovered by Olbers the 6th of March: observed last by Gauss, the 25th of August. Bessel has taken the perturbations into

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account for his elliptical orbit. Period, according to Nicolai, 74.7893 years; according to Bessel, 74.04913. Bessel computes, that taking all the perturbations into account, the comet will reach its perihelium again in as early as 1887, Feb. 9.4, that is 824.5 days earlier than the period of the simple elliptic orbit. Berl. Astr. Jahrb. 1818, 1819. Observations at Königsberg, II. Zeitschrift for 1816. Trans. Berl. Acad. 1812, 1815. Math. Class. Conn. des tems, 1820. Bessel reckons the longitudes from the mean equinox of the 1st Jan. Nicolai from the 26th April, 1815.

118. 1818. Discovered by Pons the 26th Dec. 1817: observed last at Bremen the 1st May 1818. The comet was on account of the faintness of its light very difficult to be observed, and appeared to be gradually dissolved. Berl. Astr. Jahrb. 1821. Zeitschrift for 1818. Conn. des tems, 1821.

119. 1818. Discovered by Pons the 29th Nov. 1818; afterwards by Bessel the 22d Dec. Last observed by Harding the 30th Jan. 1819. Berl. Astr. Jahrb. 1821, 1824. Conn. des tems, 1821. Corresp. Astr. II. p. 187.

(86.) 1819. Reappearance of the celebrated comet of Encke, by which its short period of 1207 days was first ascertained. Discovered by Pons the 27th Nov. 1818: observed last the 12th Jan. 1819. Only the last elliptic orbit of Encke is to be considered as the true one. Corr. Astr. 1819. Berl. Astr. Jahrb. 1822, 1823. Conn. des tems, 1822. Encke reckons the longitudes from the mean equinox of the 1st Jan. 1819.

120. 1819. Appeared suddenly in Europe emerging from the sun's rays in the beginning of July, of a considerable magnitude. Last observed in October at Dorpat and at Bremen. Is remarkable, because, according to the elements, it must have passed over the sun's disc on the 26th of June. Corr. Astr. 1819. Berl. Astr. Jahrb. 1821, 1822. Conn. des tems, 1822.

121. 1819. Discovered by Pons the 12th June, and only observed at Marseilles and in Milan till the 19th July. Only the last orbit, by Encke, agrees with the observations, which cannot be represented by any parabola. Corr. Astr. 1819. Berl. Astr. Jahrb. 1822, p. 243; 1823, p. 221. Efem. Milan. 1820. Encke computes from the mean equinox of the 1st Jan. 1819.

122. 1819. Discovered by Blanpain at Marseilles the 28th Nov.: observed last at Milan the 25th Jan. 1820. Observed also at Bologna, and especially at Paris. The great deviation of the orbit from a parabola is not to be doubted; but the limits of the time of revolution have not hitherto been found assignable, on account of the too short interval between the observations which have been published, and which are in some degree of doubtful accuracy; those of Marseilles not having been obtained by the most earnest entreaties and demands. Corresp. Astr. 1820. Berl. Astr. Jahrb. 1824. Conn. des tems, 1824. Encke reckons from the mean equinox of the 1st June, 1820.

123 1821. Discovered at the same time on the 21st Jan. by Nicollet at Paris, and by Pons at La Marlia. Was observed in Europe till the 7th March, and after the perihelium, from the 1st April till the 3d May at Valparaiso, by Captain Basil Hall, Lieutenant W. Robertson, and Mr. H. Forster. Its apparent

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motion was very slow throughout the time of the European observations. Brinkley's first elements are founded only on the observations at Valparaiso: the second he has computed from the observation of the 30th January at Bremen, and those of Capt. Hall, made the 8th April and the 3d May. Rosenberger was able to represent both the European and the American observations sufficiently well by his parabola. The rest of the orbits are founded on the European observations alone. The orbit seems to differ very little from a true parabola. Corr. Astr. 1820. Conn. des tems, 1824. Berl. Astr. Jahrb. 1824. Phil. Tr. 1822, Pt. i. Edinb. Phil. Journ. N. xiv. p. 382. Schumacher Astr. Nachr. N. 2, p. 17. N. 11, p. 166. N. 24, p. 425.

124. 1822. Discovered by Gambart at Marseilles the 12th May, by Pons at Marlia the 14th, and by the Oberlieutenant Biela, at *Prague, the 16th: observed till the end of June.* Zach Corresp. Astr. 1822, cah. iii. iv. v. Schumacher Astr. Nachr. N. 19, p. 298. N. 20, p. 309. Berl. Astr. Jahrb. 1825.

(86.) 1822. Orbit corrected by Encke from Rümker's observations at Paramatta. Schumacher Astr. Nachr. N. 27, p. 39. The longitudes relate to the mean equinox of the 24th May.

125. 1822. Discovered the 13th July by Pons, at Marlia: observed till the 22d October. Only the second ellipsis of Encke is founded on the whole apparent arc described: but the second parabolic orbit of Nicolai, and the third of Hansen represent also the whole of the observations hitherto published in the most satisfactory manner. Perhaps we may expect to receive from the Cape of Good Hope, or from New Holland, some observations of the two last comets, as well as of that which was discovered on the 31st May, by Pons, at Marlia, near β Piscium, and which was little observed in Europe, and has not yet been computed. Zach. Corr. Astr. cah. v. Schumacher Astr. Nachr. N. 20, p. 307.; n. 21. Suppl.; N. 22, p. 361, and 1. Suppl.; N. 23, p. 393, and Suppl.; N. 24, Suppl. Nicolai supposes the mean equinox of the 23d Oct.; Hansen in the second orbit, the 1st Sept.; in the third the 1st October, and Encke, the 25th Oct. 1822. [See also Gambart in Conn. des tems, 1826. Tr.]

ii. *Further Remarks on the periodical* Comet (86 Olb.) *with conjectures on the effect of a resisting medium.*

By Professor Encke. *Bode's Alm.* 1826, p. 124.

The observations of Mr. Rümker have removed every possible doubt respecting the identity of the comet, and made it certain that future Astronomers will be able to recover it even if it should remain invisible for several revolutions. Fortunately, however, there is no reason to apprehend that it will escape us in its next

visit. Dr. Olbers first pointed out to me that if it passed the perihelium later than the 10th of September, it will be visible to Europeans in August. From a cursory computation of the perturbations, I find that its perihelium will be about Sept. 16.4, and its places will be nearly these:

	A. R.		Decl.		Log. Dist.	
1825	°	′	°	′	☉	⊖
Aug. 1.6	82	31	32	1 N	0.023	0.162
6.6	90	23	32	9	9.988	0.141
11.6	99	1	31	44	9.948	0.123
16.6	108	19	30	36	9.903	0.107
21.6	118	9	28	37	9.852	0.097
26.6	128	14	25	40	9.792	0.092
31.6	138	23	21	46	9.724	0.093

Its next return to the perihelium will be in Dec. 1828, or perhaps in the beginning of January 1829: and it will then be easily visible, unless its light should prove to be gradually diminishing.

In attempting to compute the perturbations, it becomes necessary to employ higher powers of the quantities concerned than those which are sufficient for the planets, and no precautions or suppositions respecting the masses of the disturbing bodies are capable of representing the successive revolutions from each other without errors of several degrees. For example, we represent the five perihelia of 1786, 1795, 1805, 1819, and 1822, most conveniently by taking the mass of Jupiter $\frac{1}{12}$ less than that which is assigned by Laplace: but errors exceeding a whole day in the interval will still remain: and the middle three of the five considered alone afford still greater irregularities, although the actions of the planets which are neglected would have a very inconsiderable influence on this combination.

Comprehending the effects of ☿, ♀, and ⊖, the best result is obtained by increasing the mass of Jupiter about $\frac{1}{7}$: but the elements thus corrected give the perihelium of 1786 two days too early, and that of 1822 a day and a half too late: nor will any

other planetary perturbation, that can be imagined, enable us to remove this error.

I therefore think myself entitled to consider it as demonstrated, that an alteration of the supposed masses of the planets alone is not sufficient for the purpose; and in the predictive computation of the places for 1822, which has now been verified, I was induced by this conviction to assume an empirical correction proportional to the square of the time.

Such a correction of the epoch corresponds to a mean angular motion increasing directly as the time, and points at once to the possibility of an explanation of its foundation. From Newton down to Laplace, a number of the most acute mathematicians have investigated the influence which any substance, scattered through space, might have on the motions of the heavenly bodies. The resistance of such a medium would necessarily occasion a continual shortening of the greater axis, and consequently an increase of the mean motion, and a diminution of the eccentricity; while the longitude of the perihelium would undergo only periodical changes compensating each other in the time of a revolution; and the nodes and inclination, which affect only the plane of the orbit, would be perfectly exempted from its influence. Now these are exactly the changes which are observable in our comet; for the decided change of the eccentricity, in the year 1822, may at least, in great measure, be attributed to some extraneous cause: though the earlier observations would not agree so well with a rapid change of this element.

Before it had been shown by these computations that the various returns of the comet could not be reconciled to each other without some such hypothesis, Dr. Olbers had already pointed out the probability of a similar alteration in a letter to Mr. von Lindenau. He wrote again to me on the subject in these words: "The exemption of the dense and solid bodies of the planets from any sensible effects of resistance, in the interplanetary spaces, proves nothing with respect to comets, which occupying, perhaps, a volume 1000 times as great, may have masses 1000 times as small: and with respect to this comet of 'Pons,' such a resistance

seems to be almost demonstrable *à priori*: for it moves, during a considerable portion of its period, in that part of the open space of the system, in which the visible substance of the zodiacal light, or solar atmosphere, is found. It is this same comet, through the middle of which Herschel, on the 9th Nov. 1795, saw a small double star of the twelfth or thirteenth magnitude, with very little diminution of its brightness. This fact seems to demonstrate that the density of the comet bears some finite proportion to that of the zodiacal light, and that the substance causing this light may afford some sensible resistance to the motion of the comet. If then all the rest of the space surrounding us were to be considered as perfectly void and free from resistance, which I do not believe, still the substance of the zodiacal light, which does certainly exist, is sufficient to explain the phenomena of a diminution of the periodical time, and of the eccentricity of the orbit."

Dr. Olbers further remarks that this ethereal medium may naturally be supposed to revolve with the planets from west to east, so as to exhibit little or no influence on their motions, while those of the comets, being more discordant, may suffer a very material disturbance from its resistance.

[Upon a probable supposition respecting the law of the density of the resisting medium, Professor Encke proceeds to calculate the places of the comet for 1795, 1805, and 1819, in such a manner as to reduce the sum of the errors to less than half their amount upon the most advantageous suppositions respecting the planetary perturbations. For 1822 the improvement does not appear to be so considerable.]

The times of the successive passages through the perihelium, as affected by the perturbation, are assigned in this table, and those, which have been observed, are distinguished by an asterisc.

*1786	Jan.	30.1	Parisian M. T.	*1805	Nov.	21.5
1789	May	18.7		1809	March	11.9
1792	Sept.	4.1		1812	June	26.3
*1795	Dec.	21.4		1815	Oct.	12.7
1799	April	11.1		*1819	Jan.	27.3
1802	Aug.	1.9		*1822	May	24.0

The comet of 1766, which has been suspected to be the same, is recorded to have been very bright to the naked eye, and its motion was retrograde.

iii. *Comparison of the* New Tables *of Refraction with Observation.*

Refractions *observed by* Mr. Groombridge. (Coll. XIII. i.)

Obs.	App. Alt.	Fahr.	M.Corr. for 1°	Refr. B.30	Red. to 46°.5	N. A.	Error.	Ivory 50°.	"Error"
	° ′ ″	°	″	′ ″	′ ″	′ ″	″	′ ″	″
16	1 18 42.6	49.2	8.5	22 28.3	22 32.7	22 16.2	−16.5	22 10.2	−10.8
25	1 30 9.5	38.1	3.3	21 38.0	21 10.2	21 6.1	− 4.1	20 58.7	+ 7.2
17	2 0 8.1	52.9	2.7	18 11.1	18 28.3	18 28.5	+ 0.2	18 19.0	− 0.4
[44]	2 17 35.2	31.9	2.4	17 38.9	17 4.0	17 10.7	+ 6.7	17 1.5	+ 2.6
13	2 30 53.4	56.7	2.3	15 51.6	16 15.0	16 18.6	+ 3.6	16 8.5	− 2.3
9	2 41 2.2	56.3	2.1	15 18.3	15 38.8	15 39.3	+ 0.5	15 29.9	− 5.1
5	2 51 34.5	28.7	2.1	15 32.7	14 55.3	15 2.6	+ 7.3	14 53.3	+14.2
12	3 2 29.9	38.9	2.0	14 44.7	14 29.5	14 27.0	− 2.5	14 18.0	+ 2.7
10	3 6 50.9	54.8	2.0	13 53.5	14 10.1	14 13.3	+ 3.2	14 5.6	+ 1.2
8	3 53 37.5	39.8	1.6	12 17.4	12 6.7	12 6.2	− 0.5	12 0.8	+ 2.6
10	4 7 2.5	32.8	1.56	12 1.6	11 40.2	11 36.2	− 4.0	11 31.9	+ 3.5
15	4 49 8.7	38.6	1.33	10 21.7	10 11.2	10 16.4	+ 5.2	10 11.8	+ 3.8
12	4 55 27.2	43.5	1.30	10 10 0	10 5.4	10 5.2	− 0.2	10 1.0	+ 1.1
16	4 55 55.7	38.3	1.30	10 12.8	10 2.1	10 4.4	+ 2.3	10 0.2	+ 5.1
							±56.8		±62.6
							+ 1.2		+25.4
									Diff.
7	5 9 17.7	58.2	1.25	9 29.8	9 44.4	9 43.0	+ 1.4	9 39.2	+ 0.6
11	5 27 58.5	33.8	1.20	9 28.5	9 13.3	9 13.8	+ 0.5	9 11.7	+ 2.1
13	5 44 33.2	53.8	1.13	8 41.4	8 49.7	8 53.0	+ 3.3	8 49.3	+ 0.3
13	5 47 55.9	33.6	1.12	8 57.5	8 43.1	8 47.6	+ 4.5	8 44.8	+ 1.1
29	6 13 12.7	38.5	1.05	8 20.8	8 12.4	8 16.2	+ 3.8	8 14.1	+ 1.8
23	6 30 26.4	58.3	.98	7 46.4	7 57.5	7 57.1	− 0.4	7 55 1	+ 1.7
7	7 6 8.5	33.7	.91	7 31.4	7 19.8	7 20.5	+ 0.7	7 19.6	+ 1.7
24	7 22 13.6	61.8	.88	6 58.0	7 11.5	7 6.0	− 5.5	7 5.2	+ 2.3
13	8 20 21.3	39.9	.82	6 36.5	6 22.9	6 20.8	− 2.1	6 19.9	+ 2.0
	° ′ ″	Int.	″	′ ″	Int. 50°	′ ″	″	′ ″	
Br.22	7 8 13	66.0	.89	6 54.2	7 8.4	7 9.6	+ 1.2	7 8.8	
P.156	13 43 0	45.0	.47	3 57.0	3 54.6	3 54.6	0.0	3 54.9	

The mean refractions of the Nautical Almanac are here compared with the observations as reduced to the standard temperature of 46.°5 for the exterior thermometer, by taking a mean of the English and French corrections, for the reasons explained in the

thirteenth number of these collections. Mr. Ivory not having precisely assigned the temperature at which his tables may be supposed to represent the results of the exterior thermometer, the errors of the New Tables have been copied from his own comparison of these tables with Mr. Groombridge's observations, and it is obvious that the New Tables, thus compared with the phenomena, do not at least possess any advantage over those of the Nautical Almanac; though they can scarcely be said to be decidedly inferior, with regard to the lower altitudes: but at 45° it appears to be highly improbable that the refraction, at the temperature of 50°, or even of 48°, can be so much as 57.″36.

Professor Bessel has given us, in the Berlin Almanac for 1826, the results of his latest observations on the refractions near the horizon, which show, very satisfactorily, what was perfectly well known before, that his table is founded upon an inadequate theory, and that it is of no particular use where any correct theory at all is wanted. The comparative results of the different determinations may be exhibited in a short table.

Comparative Results, Barometer 30.

Alt.	N. A. 48°.5 Ext. 50° Int.	N. A. 52° Int.	Ivory	Diff.	Bessel F. A.	Bessel Corrected	Corr.
° ′	′ ″	′ ″	′ ″	″	′ ″	′ ″	″
0 0	33 51	33 34.8	34 17.5	+42.7	36 36.1	36 0	−36
0 30	28 37	28 24.8	28 40.8	+16.0	29 51.4	29 27.3	−24.06
1 0	24 25	24 15.6	24 21.8	+6.2	24 58 2	24 44.6	−13.60
1 30	21 7	20 59.2	20 59.6	+0.4	21 18.3	21 7.9	−10.35
2 0	18 29	18 22.6	18 19.6	−3.0	18 28.8	18 23.3	− 5.46
2 30	16 21	16 15.6	16 10.89	−4.7	16 15.1	16 13.9	− 1.19
3 0	14 35	14 30.4	14 26.01	−4.4	14 27.5	14 26.1	− 1.42
3 30	13 7	13 3.0	12 59.51	−3.5	12 59.5	12 58.7	− 0.77
4 0	11 52	11 48.6	11 47.15	−1.1	11 46.4	11 18.4	+ 1.97
4 30	10 50	10 46.9	10 46.03	−0.9	10 44.9	10 48.2	+ 3.30
5 0	9 58	9 55.2	9 53.84	−1.4	9 52.5	9 52.4	+ 0.05
6 0	8 32	8 29.7	8 29.80	+0.1	8 28.5	Berl. Astr. Jahrb.	
7 0	7 27	7 25.0	7 25.40	+0.4	7 21.2	1826.	
8 0	6 35	6 33.3	6 34.68	+0.4	6 33.6		
9 0	5 51	5 52.5	5 53.79	+1 8	5 52.8		
10 0	5 20	5 18.6	5 20.19	+1.6	5 19.4		
15 0	3 34.3	3 33.4	3 31.70	+1.3	3 34.8		

Alt.		N. A. 55°.4 Ext. 50°. Int.	N. A. 52° Int.	Ivory	Diff.	Bessel F. A.
°		′ ″	′ ″	′ ″	″	′ ″
20	0	2 38.[illegible]	2 38.1	2 39.16	+1.2	2 38.9
25	0	2 4.2	2 3.7	2 4.65	+1.0	2 4.4
30	0	1 40.5	1 40.1	1 40.85	+0.7	1 40.7
35	0	1 23.0	1 22.7	1 23.25	+0.5	1 23.1
40	0	1 9.3	1 9.0	1 9.59	+0.5	1 9.4
45	0	58.1	57.9	58.36	+0.5	58.27
50	0	48 8	48.6	48.99	+0.4	48.91
60	0	33.6	33.5	33.72	+0.2	33.67
70	0	21.2	21.1	21.26	+0.16	21.23
80	0	10.2	10.2	10.30	+0.10	10.29

The results of the Nautical Almanac are reduced to 52°, in order to compare them the more readily with those of Mr. Ivory.

For the mean probable error of a single observation, Professor Bessel and Mr. Rosenberger have found at

Alt.	Error	Alt.	Error	Alt.	Error
° ′	″	° ′	″	° ′	″
0 30	20.0	2 30	5.3	4 30	2.0
1 0	16.8	3 0	3.9	5 0	1.7
1 30	10.6	3 30	2.6	10 0	0.9
2 0	7.7	4 0	2.4	45 0	0.3

And the probable ultimate errors of their determination of the mean refraction, at 1° and 2°, are found to be 2″.5 and 1″ respectively.

With respect to the refractions *below* the horizon to which the table of Bessel extends, it will always be amply sufficient to take the *mean* horizontal refraction, and to increase it by its excess above the refraction computed for an altitude equal to the depression, and for the *actual* state of the atmosphere; except that if the temperature at the surface of the sea were known to be elevated or depressed, it would be proper to correct the mean horizontal refraction accordingly.

The whole of this comparison has been instituted in order to ascertain the propriety of retaining or suppressing the remark subjoined to the table in the Nautical Almanac, that it "appears to agree more perfectly with the latest observations than any other table that has been published," explained as it is by the admission in the preface, that the "deviation from the French tables in the

mean value of the refraction," is altogether inconsiderable. If the new table had been decidedly shown to exhibit results more exactly conformable with the actual determinations of astronomers than those of the Nautical Almanac, it would have become the duty of the editor to adopt them; but for the present there can be no question that such a change would at least be premature.

iv. *Note on* Refraction, *addressed to Professor* Schumacher.

My dear Sir,

I was much surprised the other day to observe, that in copying the explanation of my Table of Refractions from the Nautical Almanac, you had omitted, without assigning any reason, the words "which would be more consistent with the theory;" an expression which I had employed in speaking of the use of the external thermometer, in preference to the interior.

I am the more disposed to remonstrate with you on this occasion, because I observe that a great number of astronomers, and among them some who do not usually act without reflection, have inconsiderately taken it for granted that the correction ought to be made according to the height of the interior thermometer, as nearest to the place of observation.

Now, with regard to the theory, it is perfectly obvious, that the computation extends only to such changes of density as take place between the different strata of the atmosphere considered as *horizontal*; and that its results must necessarily terminate where this regular constitution of the atmosphere ends; that is, *outside* the observatory, or other building, containing the instruments; while the change of density between the external and internal air, taking place in general at surfaces more nearly vertical than horizontal, at least when the object is but little elevated, and certainly never at horizontal surfaces, will either have no effect at all in increasing the refraction, or as great an effect at higher as at lower altitudes; so that this little irregular addition or diminution can never require to be considered as a part proportional to the whole original mean refraction.

With regard to practice and observation, I need only refer to

Mr. Delambre's remarks in the *Connaissance des Tems*, for 1819, where he shows that for Mr. Groombridge's observations, the mean error of the exterior thermometer is only five sixths as great as that of the interior.

v. *General Results of Observations on the Dipping Needle.* By W. Scoresby, Jun. Esq.

1823	Time	Place		Mean dip.
Mar.	29	Liverpool		71° 33′ 0″
June	10	La. 71° 31′ 14″ N.	Lo. 12° 7′ 15″ W.	78 3 6
July	5	71 38 0	17 37 0	79 0 7, or
				79 6 7 (M.)

The mean dip is the mean result of observations with and without a sphere on the needle: the last result, (M.) is obtained by the formula of Professor Mayer.

vi. *Elements of the* Comet *of* 1823-4. *By various Computers.*

1. The first received by the Editor were from Mr. J. Taylor of the Royal Observatory, Greenwich.. 2. The second are by Professor Nicolai Schumacher, Astr. N. N. 48. B. 3; giving the greatest error in A. R. + 18″, in decl. + 11″. 3. The third by Mr. Hansen, A. N. 48, B. 3. 4. The fourth by Carlini. 5. The fifth by Dr. Brinkley. 6. The sixth by Mr. Richardson, of Greenwich.

Passage of Perihelium	1.	1823 Dec. 9.3697ᵈ Greenwich					
	2.	9.4380 Manheim					
	3.	9.47193 Altona					
	4.	9.4792 Greenwich					
	5.	9.2168 Greenwich					
	6.	9.4521 Greenwich					
Longitude of ☊	1.	302° 56′ 34″	4.	303°	4′	4″	
	2.	303 1 18	5.	303	0	40	
	3.	303 3 22	6.	303	1	43	
— Perihelium	1.	28 43 54	4.	28	26	8	
	2.	28 43 46	5.	29	18	50	
	3.	28 29 55	6.	28	20	6	
Log. nearest distance	1.	9.3598242	4.	9.3545000			
	2.	9.3579600	5.	9.3689400			
	3.	9.3553934	6.	9.3536855			
Inclination . . .	1.	76° 55′ 45″	4.	76°	12′	50″	
	2.	76 9 40	5.	76	1	40	
	3.	76 11 22	6.	76	8	28	

Motion retrograde.

ART. XII. ANALYLIS OF SCIENTIFIC BOOKS.

Sur les Ichthyolites ou Poissons fossiles. Par Monsieur Blainville. *Article extrait du Nouveau Dictionaire d'Histoire Naturelle.* Vol. 28. *Paris,* 1822.

THE reputation and the success of Cuvier in that department of Natural History which respects the animals of a former state of the globe, and his comparative omission of that branch of those researches which relates to fishes, seem to have stimulated the author now before us to the present undertaking. The ambition is laudable, but we fear that we cannot say the same of the execution. Though this is far from being the first work which has pretended to treat of fossil fishes, it is the first which has been exhibited as a complete system of the present state of knowledge on that subject. That it is both faulty and imperfect, is sufficiently obvious; and we are much inclined to think, that if Cuvier had considered the subject as capable of being undertaken to any purpose, he would not have left it to his ambitious imitator.

We fear we must say that this is an instance, among many now too common, of that desire to shine or glitter in a new science, which is the disease of the day, and which has rendered geology the victim as it is the butt of every tyro, who, incapable of dealing with the accurate sciences, hopes to acquire some notice by evacuating on the unlucky public, the records of observations and speculations which, like their authors, have scarcely cracked the shell, and are desirous of flying before they have learned to walk.

There are reasons however for our faith in all cases; and we owe Monsieur Blainville the politeness of shewing what his claims to authority on this question are. The work must rather be considered a compilation from the writings and observations of others, than an original production; and therefore the authorities are rather those of Scheuchzer, Haller, Volta, Faujas de St. Fond, Lametherie, Lamanon, Cuvier, and others, than those of the writer himself. Thus far, different measures of confidence will be allotted to them: confidence regulated, partly by the weight of the several persons quoted, and partly by the state of knowledge of the periods at which they severally wrote. Besides, and in aid of this, Monsieur Blainville has had access to the collections in the Museum of Paris, and thus has had the power of comparing, in many cases, the printed descriptions with the specimens.

But somewhat more than all this is required for a just or luminous compilation on such a subject; and here, our author, we are sorry to say, is entirely deficient. It is one thing, even admitting that could be done in the present case, to ascertain the comparative anatomy of

the animals in question, and another to assign them truly to their geological situations. It is not sufficient that every fragment of bone, whether of land animals or of fishes, should be referred to an individual species or genus; but it is most essential that the true geological situations in which they occur, should be accurately understood and accurately described. We do not want to know merely what animals have existed, but when, where, and how they lived. Doubtless, it is important to know that many animals lived at all in a former state of the world, which are living in it no longer; it is important to know what these were, and how many; what species and genera have disappeared. This is a question however, which, abstractedly taken, concerns zoology only. The geologist is anxious for much more. He desires to know at what period of the globe they were in existence, in what lands or waters they lived, when they were buried and preserved, and how. And he desires to know all this, because he makes use of it as evidence respecting the history of the globe and its revolutions. Hence, he ascertains, or at least adduces collateral evidence towards ascertaining, the nature, and order, and places, and comparative times of its revolutions, and thus he acquires knowledge which, judiciously combined with the history of the mere rocks themselves and their various phenomena, enable him to make nearer approximations to a true theory of the earth.

It is indispensable therefore that the comparative anatomist should in this case be a practical, expert, judicious, and experienced geologist. He should be as replete with sound logic as he is free of system; should be as accurate an observer of geological facts as well stored with observations; and should be able, from his general knowledge, to exercise a critical and sound judgment on the reports and observations in zoology as in natural history, of those from whom he is compelled to borrow what he has not possessed the means of ascertaining from personal observation.

We wish we could say that Cuvier, much as we respect his soundness of mind and minute knowledge in comparative anatomy, were able equally to stand this test in geology. We wish we could say this in a far minor degree of Monsieur Blainville, but he is no geologist. Judging from his book, we are entitled to say that he has as little knowledge of this important part of his duty as is well possible. He is no observer, and he cannot be a critic. Hence every thing from which the geologist ought to have derived assistance, all that he would have turned to for light, only leaves him in darkness, the same or worse than before.

Monsieur Blainville copies, without discrimination, from the description of those who wrote before geology had been rescued from its ancient state of night and chaos; and, unable or unwilling to verify or rectify the observations of his remote predecessors, leaves every thing where he found it, or rather, adds to the confusion which pervades their remarks.

Such a work could not have been executed as it ought to have been, either in Paris or in the Paris Museum. It ought not to have been attempted, but by him at least, whose experience in geology rendered him competent, from other knowledge, acquired in other places, to verify the probable truth or detect the fallacy or imperfection of the reports of places which he was unable to visit. That the attempt has consequently failed in its most essential part, is but too plain.

We wish that what we have said, (and we might say much more did our limits permit,) would impress, not only on our neighbours but on the geologists of our own country, the necessity of keeping a steady regard, in their investigations, on the ultimate purposes to which these ought to tend or be directed. Geology itself, the history of the globe of the earth, is a difficult, severe, abstruse and laborious study. It requires much personal labour, much time, much acuteness, some reading, much freedom from system and prejudice, and an earnest desire for truth; with a cautious, rigid, severe, logic, and trained habits of a close and strict reasoning, which partakes often as much of moral and metaphysical, as of mathematical thinking and induction. It is not the collecting of specimens, or the forming of sections in the closet, and of coloured maps from the imagination, or from much conjecture and little observation, which constitute geology; and, this abstracted, there is little in it to satisfy the craving desire for ease and amusement united, and for some poor temporary fame to be acquired by papers in transactions and systems of Scotland or Siberia, on which the dilettantes in science live. Hence the labour is shunned; and the study evaporates in the far easier task of collecting bones and shells, in marvelling at the crocodile and rhinoceros which occupy the place since held by the two kings of Brentford, or at the hyena who proves the hardness of his jaws upon the bones of Yorkshire rats, and at the nature of *Album Græcum*.

But we must reserve our general criticisms on the present state of this science for a fairer opportunity, and return to Monsieur Blainville.

We have said that he was no geologist, and that he was incompetent to his subject, because deficient in that most essential part of it. But we have a serious objection also to make, to the other department of his work; to the rigidly zoological or anatomical part of it. All the world has marvelled, and with some reason, at the ingenuity with which Cuvier has contrived to erect new genera and species, and to produce entire animals which were never yet seen, and never will be, from fragments of rotten bones; constructing a Megætherium from a maxilla, and a Hyena from an os hyoides. With this we have nothing at present to do; satisfied with the ingenuity of the Zadig of the day, and, as far as authority can avail, quite as willing to allow him the dictatorship in this matter as any other person. There is always a latent delight in surrendering ourselves to the marvellous.

But even to this delight there are bounds; and when these are exceeded, we are apt to feel a twinge of the "incredulus odi." Unques-

tionably, the skeleton of a fish may be good evidence for the fish itself, as far as we may be satisfied without regaling on it, or are contented with guessing how it might have looked in a drawing, or skinned, varnished, and stuffed with plaster of Paris. This mode of assigning a species or a genus, will be still more satisfactory, when the naturalist has had the means of comparing the preserved skeletons of former days with those of existing species and genera to a sufficient extent. But who need be told that there is such a simplicity and general uniformity in the skeletons of fishes, that the limits to this mode of investigation are very narrow indeed. They have no legs nor arms, no scapulæ nor knee-pans, no os coccygis nor sternim, nor phalanges, nor any of those multitudinous and ever varied parts from which the comparative anatomist derives so much facility in his researches on quadrupeds. There is something in the number of the spinal bones, there is something in their forms and proportions; and there is still more in the bones of the fins and in those of the skull. But all this is little; and while but little evidence can be derived from fragments, we are particularly determined to distrust Monsieur Blainville on points which neither he nor any one else could have ascertained, namely, the erection of new genera and new species from the contemplation of fragments, and these fragments often distorted by the effects of pressure and the other causes of change and injury to which fossil bones are exposed.

When we said that Monsieur Blainville was ignorant of geology, we might also have said that he does not seem to have formed any conception of its nature and meaning, and of the relations of his own subject to it; considering this, as we do, rather a branch of zoology than of geology properly so called. He speaks as if the strata were only casual substances which might or might not be studied, but as being " often useful." We would gladly know how they are not always necessary instead of being often useful, at least in our view of the subject. If the object is merely to ascertain lost animals, they are neither necessary nor useful; whence it is plain that when M. Blainville speaks thus, he is thinking of geology, not of his fishes, and thinking too, to little purpose.

When he asserts that the nature of the organic remains offer the most " unequivocal methods of establishing geology on indisputable bases," he is only saying what others have said before him, but which is not a bit the more true because it has been often said. In the first place, we will admit this, and then ask to what extent the science of geology can be based on the knowledge of organic remains? In many countries they do not exist; in many rocks they never occurred. They are limited to a small portion of the geographical globe, and they are confined to a small depth of the geological one. What would become of the theory and history of the primary rocks, of the trap rocks, of the volcanic rocks, if their history and theory depended on their organic remains? The organic remains

are connected mainly with the last revolutions of the globe;—universally with the later ones. And yet among these later, we must except the latest of all, which are the whole of the two classes of rocks produced by fire, the traps and the volcanic rocks. As to all the rocks which precede coal, with little exception, we should never obtain any knowledge of their theory, did we depend on the evidence to be derived from organized bodies. That they prove many things, is unquestionable: but it would be a defective system of geology indeed, and we might well despair of attaining any knowledge of that science, if we had no more knowledge and no wider views than Monsieur Blainville, and, (we might add,) many more, seem to possess on this question.

We are equally ready to deny, and to prove it, had we room, that even the order of the succession and the true theory of those very strata in which organic remains exist, can be proved by means of them. This has been a favourite theory to the present day, and it has a large body of abettors still. But we could prove, by their own evidence and shewing, that it is unfounded; by quoting their own catalogues of the strata and their included shells, and by shewing that the same genera, and the same species in many cases, occur through all the series, in positions the most remote. We could even prove it *à priori* from zoological considerations. Were the assertion true in geology, or in organic mineralogy, (to use a better phrase,) then it would have been a necessary preliminary that all climates should have produced, at different remote times, similar families of animals; that all these should have followed each other in a certain unvarying order, and that the same order and kinds should have existed and succeeded every where in one manner. It would have been impossible that there should now have been, had the same laws prevailed formerly as now, oysters at Milton, and muscles at Hastings, and cockles at Margate, and periwinkles at Dover.

But we have not time for what well deserves a separate discussion; and having thus far disputed Monsieur Blainville's preliminaries, shall proceed to make a few remarks and extracts from a book which we might have easily disputed at every page.

As we cannot afford to quote a great deal, we must try to be content with a few passages, and shall take the following in the first instance. We insinuated this author's want of logic; and surely it was an unjust insinuation, since the arrangement would do justice even to Jeremy Bentham. There is a Tudesqueness in it which is quite delightful, and which bespeaks the genius of a German professor crazed with the logic of Kant and Burgersdyck, and the reading of the schools, rather than the œstrus of a lively Parisian skipping through the dry bones of the Musée. If it is a long passage, we can only say in its defence, as Horace Walpole did after Gray, and of other passages, that it " leads to nothing."

"Sous le rapport de la composition chimique ou anatomique," (he is speaking of organic fossils.) J'ai divisé les corps organises fossiles; A. en ceux qui n'ont éprouvé aucun changement dans leur tissu, dans leurs compositions chimique et mineralogique; B. en ceux qui ont perdu seulement et entierément la matiére animale; C. ceux qui ont la même composition chimique moins la matiére animale, mais qui ont perdu de plus leur structure et leur forme; D. ceux qui n'ont perdu que la partie organique, mais dont la portion inorganique a pris une disposition toute différente *de la Spathification;* E. qui ont éprouvé des changements dans la tissu anatomique et dans la composition chimique, méme dans l'acide du sel terreux qui les fermait; F. ceux qui n'ayant rien perdu dans la structure organique, ont été entiérement changés dans la composition chimique; *de la Petrification:* G. fossiles qui sans avoir éprouvé de modifications ont été imprégnés d'une substance metallique; *de l'Impregnation;* H. des corps incrustés; I. des corps succinisés."

This reminds us of a modern Act of Parliament: a trap set to catch all the modifications of possibility, and somewhat more; but which is still so ill-constructed, that lawyers, rats, and criminals contrive to escape it.

A German engrafted on a Parisian, forms a heteroclite-enough animal; somewhat, we should conceive, like Monsieur Blainville's own palæobalistums and palæorynchuses. But such is the consequence of going to school at Freyberg. We thought that the Pope had been dead, for, like his namesake in John Bunyan, his nails had been pared some time ago; but it seems that his ghost still walks. The earth, says our author, is divided and subdivided. This is highly instructive; and firstly of the second, which are the organiferous strata, and of which the subdivisions are thus stated. We would have translated this logical and luminous passage for the benefit of our English readers but we want words. The divisions in French therefore are, "1re. Terrains zootiques les plus antiques, trés antiques et antiques: 2me. Terrains les plus anciens, trés anciens, et anciens: 3me. Terrains modernes, comme d'alluvion, des tourbiéres: and lastly, 4. Terrains recens ou terrains meubles et couches superficielles." We have condensed the quotations, and hope they are the more intelligible. As to the first or the grand division, it is zootique and azootique. It is a fine thing to understand Greek and Logic; and the author's positive, comparative and superlative, antic and ancient strata, remind us of the "heavy not particularly light," and the "intermediate between hard and semi-hard inclining to the soft," with the "scopiformly divergently radiated," and so forth, which argue the metaphysical and delicate profundity in language and thinking, which distinguishes another of the luminaries of this science.

But we have said enough of this author's general views, and must give a few specimens of his details. Of these, after the geological

confusion which we have already sufficiently noticed, the leading character is the ambitious desire of creating new species and genera; apparently, with the design of rivaling Cuvier in his own peculiar walk, and for the sake of displaying his profound knowledge of Greek. As this language constitutes rather a novel science in France, we must excuse Monsieur Blainville for his wish to prove that he is actually the possessor of a Greek Lexicon.

Whether the strata of Glaris are to be considered as most antique or very antique, or antique, or whether they are most ancient or very ancient, or ancient, to which of these two sets, in short, of positive, comparative, and superlative entities they belong, Monsieur Blainville has not told us, and we cannot guess. But we must try if we can conjecture to what rank they belong in the vulgar language. We sincerely wish that geologists would use the same words as other people, to express such ideas as they happen to possess. If matters proceed much further in this way, what with German nomenclatures and French nomenclatures, books on geology will become as intelligible as the treatises for digesting sol with luna under the red dragon per "pemset remsen ame muc senvu saltrafi."

Scheuchzer, as well as Ebel and many others, have examined this place, and many collections have been formed from it. It is situated to the south-east of Glaris, in a small valley, at a distance of about five miles, in a part of the mountains called the Plattenberg. The including rock is a blackish fissile schist, containing some mica, and interstratified with thin laminæ of limestone. It is, as he says, the Grauwacké schiefer of the Germans, and, what is worse, the Phyllade paillctée of Brongniart, as if one hard name was not enough. The specimens here are very imperfect, being only the impressions of the fragments of skeletons; one side of which has formed a sort of bas-relief in the schist, while the other is very ill-defined.

Now Haller, a name not to be spoken of lightly, even by Monsieur Blainville, mentions the impressions of ferns as being found in the same places; but this he thinks improbable, because Brongniart found none in the collections which he examined, and chooses to call it a transition rock. Here the question of the geological nature of this deposit becomes most important. Our author, making up his mind that it is a marine formation, determines that all his specimens are sea-fishes. We have abundance of respect for Brongniart, but have also good reasons for not giving implicit credence to his geological opinions. Cuvier thinks it is marine, because it contains the remains of a tortoise, and because that tortoise must have been a marine one. We should be very glad to know whence this necessity arises: there was formerly the same compulsion on all the Lacertæ, the crocodiles, to belong to fresh-water; but, unluckily, Lieutenant Kotzebue finds that there are sea crocodiles in the Pellew islands. Here then we have a contest of evidence: the ferns which Haller saw, against the tortoise that *must* have lived in the sea; and, further,

the opinion of Andræa, who says that it is a fresh-water formation, against that of Brongniart. We do not intend to decide between disagreeing doctors, but it is a justice to our readers, if they are readers also of Monsieur Blainville's Ichthyolithology, to dissect these paragraphs for their use.

Eight species of fish are described as being found here. Of two of these, Monsieur Blainville makes new genera, by the names of Anenchelum and Palæorhynchum; the others are supposed to belong to Clupea and Zeus. The first of these was formerly imagined to be an eel; and although that opinion was probably wrong, we cannot see, how, from the miserable evidence which the fragments are admitted to afford, it is possible to make a new genus for it. But this naturalist finds less difficulty in constructing a genus out of a fin or a tail, than Linnæus did with the whole living races before him: according to the well-known adage, "Qui ad pauca respiciunt de facili judicant." Here is the way, for example, in which Palæorhynchum (old Snout, for the benefit of the unlearned) is made. "Quoique cet Ichthyolite, dont nous n'avons vu que *la figure de la partie anterieure*, dans l'Herbarium diluvianum tab. 9, fig. 6, nous soit trop insuffisament connu pour appuyer notre opinion, il ne nous paroit nullement probable que ce soit notre aiguille; (Esox bellone,) ainsi donc, jusqu' à des circonstances plus favorables, nous proposerons de la designer provisoirement sous le nom de Palæorhynque de Glaris." We shall really be glad to know how such trifling as this can conduce to the study of ichthyology, or geology, or any other Ology in the whole circle of the sciences.

Scheuchzer takes another of these fishes for a bleak; not an unlikely conclusion, if this same deposit contains ferns; but our author chooses to make it a new clupea. A fourth was esteemed a pike, and this also he makes a clupea; which judgment being deduced, not from a specimen, but from a figure by Knorr, it is very satisfactory to be informed that it is uncertain whether that appertains to the rock of Glaris or not. On such principles as this, we are likely soon to abound in Ichthyolitologists and Ichthyolitologies. There is a third clupea, with a new title also; all of which is matter of course, since it was predetermined that this was a marine formation.

Next comes the genus Zeus, of which he finds three new species. To show how satisfactorily these points are settled, in the first place, the first species is determined to belong to Glaris, not because it was found there, but because it lies in a similar slate: as if similar rocks of all kinds were not found all over the world. This may very well be a marine fish, if he pleases; but how does it follow that it has any thing to do with Glaris, or that it proves this to be a marine deposit? As to its own characters, it has "des rapports avec le Zeus, ou genres voisins; mais c'est ce qu'on ne peut assurer, parce que la tête toute entiere manque." Then "toujours est il constant que c'est un poisson marin." Very possible. And so for the others.

The fossil fish of what is called the metalliferous slate, are well known to be abundant, in numbers at least, if not in kinds; and they occur in many different places. The most noted of these are the Palatinate, the Voigt, and Thuringia; and they have often been described by different authors, such as Kruger, Friesleben, and others. It is somewhat remarkable with respect to these specimens, that they are almost always much distorted and injured; not even being compressed and preserved laterally, as is the most usual case in the fossil fishes. It is equally so, and important at the same time in investigating the species, that the impressions are those of the skin and substance of the animal, not of the skeletons.

With respect to the nature of this deposit, we have no objection to be convinced that it is a marine one, if it appears that any one of the fish is marine, or that any one sea-shell is contained in it. It may very probably be so, although no such evidence is produced. But if we are willing to believe quietly and without any evidence at all, we do not choose to be obliged to believe by that which is not evidence; and this, not on account of any concern we feel about the bitumino-metalliferous schist of Mansfeld, but because of the very testimony itself. We have a mortal aversion to corrupt evidence in all its modes, and do not choose to pass any attempts to introduce any more of it into geology, where there is already an abundance.

These strata are determined to be marine because they lie beneath calcareous rocks containing "ancient" (or modern) marine shells, such as belemnites, entrochites, and ammonites, of the same kind as those that belong to the limestones of the Apennines and Alps, together with gypsum accompanied by sea-salt, gypsum without sea-salt, sandstone, and so forth. Now these strata are the exact counterparts of the red marl and lime of England; particularly where they are somewhat intermixed. We have no objection, either to their marine origin, or to their antiquity, if that will give Monsieur Blainville any satisfaction; but neither of these will prove that the strata below them are of marine origin also. Our own coal strata are situated in this very position; and no one now, it is hoped, since the theories of Dr. Hutton and of Kirwan on this subject are forgotten, will imagine that a series which contains terrestrial vegetables in abundance, and which never was known fairly to include a sea-shell, is of marine origin. Thus much for what is possible respecting this deposit of ichthyolites.

Monsieur Blainville has made twelve species and three new genera out of this collection. The new are palæoniscum, palæothrissum and stromateus: the old ones clupea and esox. In general we may remark on these determinations, that they are more free from objections than some of the preceding; as the author had access, in many instances at least, to more perfect specimens. How far, however, his arrangements are justified, we cannot pretend to decide. We may

nevertheless remark, that he has here attempted to give generic characters, which, in some other cases, he has oddly enough seemed to have thought unnecessary. Surely if a genus or a name is to be erected for an ichthyolite, or any other kind of lite, the purpose is, if there be any purpose at all, to allow others to refer to it, and to arrange their discoveries under the appropriate division. If this is not to be, if there is only to be a palæo—something, without characters, we do not see what natural history, ichthyology, or geology is to profit by such a coinage of crabbed words; while we do see that, on the same principle, we may shortly have as many genera as there are specimens; a proceeding likely to be attended with no convenience that we know of to compensate for the vexation of such a catalogue, except that of inducing gentlemen to turn the leaves of the long-forgotten Lexicon; that unlucky book, thumbed in the Anglo Greek division by every projector who wants to dazzle our understandings with a Diatalaiporou, a Therapolegia, an Anthropomonotroche, or an Apolepsia alexiacon.

Of these new genera we shall give the characters of that in which our author has been most successful and appears most justified, as a compensation for some of the others which we have noticed. "Palæothrissum." "Il a pour caractere essentiel: d'etre abdominal, malacopterygien, de n'avoir qu'une seule nageoire superieure située devant l'anale, entre les pelviennes et elle, et surtout d'avoir la queue bifurquée, et le lobe supérieur ordinairement beaucoup plus long que l'inférieur, et couvert d'ecailles dans toute sa moitié supérieure." There are four species of this; but Kruger thinks that one of them is a pike; it is doubtful if two of the others are not the same, though each one has its own name; and, what is worse, the geological relations of the rock in which these are found is doubtful.

That one which follows is called stromateus; but, as is most usual, has no generic characters assigned. How species are to be established before a genus is determined, rather surpasses our comprehension; nevertheless there are three, with the names major, gibbosus, and hexagonus, (surely the love of arraugement is a terrible disease,) and two more without names, the genera of which, are, however, left in doubt. We shall extract a few words from these descriptions, that naturalists may see what marks they may have to deal with when they take to the investigation of ichthyolites.

No. 11.—On trouve encore à Eisleben une autre espèce d'ichthyolite, mais qui jamais, ou fort rarement est entière: d'après la grandeur de sa tête, on suppose quelle pouvoit avoir près de trois pieds: sa peau etoit, dit on, comme chagrinée. D'après cette indication, je supposerois volontiers que les oryctographes indiquent par la un poisson fossile, dont j'ai vu l'empreinte d'une partie de la peau dans la collection de Monsieur Brongniart. On y peut reconnoitre à ce qu'il m'a semblé, une assez grande nageoire dorsale; mais ce que

cette peau offre de remarquable, c'est d'etre entièrement récouverte d'espèces de petites écailles, comme trifurquées a leur pointe, et qui semblent formées par deux chevrons disposés en sens inverse.

Je n'essaierai aucune conjecture sur le genre de poisson à laquelle cette peau a appartenu; mais je ferai l'observation que l'espèce d'ecusson qu' on voit souvent à la racine des nageoires, dans les fossiles que j'ai designés sous le nom de Palæothissum, ressemble beaucoup a ces sortes d'ecailles."

We have no objection to this manner of contemplating the subject. It is proper that specimens, be they never so imperfect, should be preserved, and figured, and described; because by the comparison of fragments at some period, a species or a genus may really be determined: it is not often that our author is so moderate: and, to continue, we shall give his equally prudent remarks on No. 12.

"Enfin, on cite encore, comme d'Eisleben, quelques restes, dont la peau est lisse comme celles des anguilles. Je crois avoir vu, dans la collection de Monsieur Brongniart, l'empreinte d'une portion de peau, qui a du appartenir à cette espèce. Le peu que j'en ai observé, et qui me paroit provenir des environs de l'anus, indique évidemment un poisson anguilliforme: toute la partie superieure offre des stries nombreuses verticales; et l'autre moitié ou inferieure, est couverte de trés petites ecailles, fort luisantes, serrées, ovales, qu'on ne voit aisément qu'à la loupe."

So much for the fishes of this deposit. But we must add that Leibnitz thought that he had found in it a mullet, a perch, and a bleak; Kruger also describes a pike; so that it may yet be a doubt whether these are marine or fresh water fishes; because, even if we were to grant Mons. Blainville all his new genera and species, it does not at all follow that they are marine ones. A word or two on this part of the subject will not be misplaced; as the determinations of our modern zoo-geolists on many parts of their investigations are very mainly and materially guided by certain notions which they have formed respecting the distinctive character of marine and fresh water species.

Let us put the very simple case that the salmon, the sea-trout, the sturgeon, or the sterlet, were found in a fossil state, we should be very glad to know how it is to be determined whether these are marine or fresh water fishes; they are both the one and the other.

But we will carry the matter a little further and say, that there are no marks in the anatomy or natural characters of a fish by which its habitation can be known *à priori*. It is a pure matter of experience now; and there is no experience about these ancient animals. For any thing to the contrary that we can ever hope to prove from natural characters, these ichthyolites may have been the inhabitants of fresh waters, or of the salt ocean, or of great inland lakes, such as are the lakes of Switzerland now, or such as the basin of Paris assuredly was long ago. In the same way, geologists have attempted to decide

upon fossil shells, inhabitants of the sea and similar objects, the natives of fresh water. That also in matter of experience, and of that only; and of the past, the so long past, there can be none. There is no character by which these can be recognised: it is not to be found in their tenderness, or the reverse, as once imagined. It is not found in generic characters, because there are species in one genus, some of which are inhabitants of the sea, and some of lakes and rivers, just as much as there is a sea eel and a river eel; a Muræna anguilla and a Muræna conger. Indeed with respect to the shell fishes, Mons. Freminville has lately shown that sea and fresh water kinds all live together in the same place. But we need not pursue this point further, and shall return with Mons. Blainville to his next geological division, the ichthyolites of what he calls the "Calcaire compacte."

As his method of division is geological, we think it would have been as well if he had satisfied his readers first of the propriety of his geological arrangements. "Calcaire compacte" may mean a great deal. The geological characters of the former strata, were merely doubtful: those of the present cannot possibly be right. The first locality, for example, is Granmont, situated at four leagues frem Beaune in Burgundy; and the rock is the "calcaire ancienne, contenant des gryphites et des belemnites," which is "situé audessus du gres rouge, et que l'on croit presque aussi ancien que celui du Jura." The next is Italy, where, without any other evidence than the colour and look of the detached stone, one is decided to belong to the Apennine limestone; we have no hesitation in admitting that some of them actually do so.

If "calcaire compacte" is to comprise such rocks as these, and if it is thus to be considered as one geological formation, we ought to have been furnished with more accurate geological information respecting them, that we might have judged of the propriety of this arrangement. If there is any object in dividing the ichthyolites according to the strata in which they are found, it is for the purpose of inquiring into the somewhat interesting question of their relative antiquity. This is what Cuvier has properly done with respect to the Paris district; and Mons. Blainville, while he was about it, might as well have imitated him in that too, had he not been solely occupied on fish bones, thinking, doubtless, "in tenui labor at non tenuis gloria." But we must inquire about the Ichthyolites themselves.

There is first a new Elops, the macropterus, from Granmont, which may or may not be an Elops; and then there is another called incognitus, imbedded in a "pierre calcaire dure, assez compacte, grise, et formant une sorte de noyau dont j'ignore la localité et le gissement." This is not a very accurate geological arrangement at any rate. The fish of Italy are left pretty nearly as they were found, but our author takes, or makes, an opportunity of cutting off Brieslak's head with a golden hatchet. We cannot pardon Brieslak any more than M. Blainville, because he has a troublesome way of thinking for

himself, and of professing that he does not understand the mystical language in which the French geognosts shroud their oracles:—"Brieslak fait l'observation, qui ni a été confirmée par Mons. Menard de la Groye, que on ne trouve dans cette localité qu' une seule espece de poisson fossile, que l'on regardé à Naples, et même parmi les savans, comme analogue du sparus quatracinus, appelé dans cette ville, sparaglioni. Comme l'observateur dont je viens de parler, en homme qui sait agir dans ces sortes de recherches, a raporté à la fois ce fossile et l'analogue présumé, j'ai pu, grâce a sa complaisance, m'assurer que ce rapprochement est tout-a-fait erroné. En effet, le poisson fossile me paroit appartenir au genre Zee, ou à l'une des subdivisions qu' y a introduites Mons. La Cépede; aussi la hauteur de son corps surpasse la moitié de sa longueur, tandis que, dans le sparus quatracinus, elle est environ le tiers." So much for Breislak and his sparaglioni.

The next geological formation is the chalk, which includes Brussels, Maestricht, Paris, and Perigueux. The first ichthyolite mentioned affords a good specimen of ichthyolitologistical reasoning. M. Burtin begins by giving "des figures assez bonnes." M. Blainville "n' en a pas vu lui meme," therefore it is, first "Zeus auratus?" "que je croirois volontiers du genre Pleuronecte, et peut être la Barbue, ou mieux encore le poisson de St. Pierre." John Dory after all. But then M. Burtin, who has drawn this very "figure assez bonne," sees fins, and ears, and sculls, and jaws, and teeth, and orbits, and clavicles, and scapula, and vertebræ; while M. Blainville sees "rien de tout cela dans la figure." And then M. Burtin "veut je ne sais trop pourquoi," that this is a Chætodon. But enough of the chalk formation. There is more of the same kind of useful information respecting the "formation du Calcaire grossier, inférieur au gypse." Cuvier seems to have been too wise to attempt it, and we shall spare our readers the sparus that may be a labrus.

Then succeeds an account of the fishes of Pappenheim, but we cannot afford to enter on the details in the same manner. The only remark we shall indulge ourselves in making, is, that in describing five species in the genus clupea, which seems a particular favourite with our author, he has borrowed from Knorr's figures, instead of consulting the specimens themselves. Thus the probabilities in favour of truth are, that, in the first place, Knorr himself is correct; next, that his painter has figured impressions of fish bones so accurately, when the value of the subject was not understood, as to enable M. Blainville to determine different species of clupea, and the genus itself from them; and, lastly, that the author has no favourite system respecting his genera, the contrary of which is evinced in every page of his work. The proof of this latter is, that all the figures which do not chance to suit the fashion of the moment, are pronounced bad; and that when they happen to suit it, they serve the purpose, with him, of demonstrating what such figures are totally incapable of proving.

His rage for maintaining his system at all hazards, is equally proved by another decision in this very case of Pappenheim. He finds a figure in Knorr, but without a locality assigned; yet he determines that it must belong to this spot, because it seems to lie in a similar stone. We should be glad to know how any figure can represent a stone, so as to render its locality certain, or even probable. We are sure that no figure of Knorr's is capable of distinguishing any one stone from another, far less the slates of Pappenheim from the slates of Shropshire or America. If thus M. Blainville's geology is studied and ascertained, we cannot have too little of it. As to the general geology of this celebrated spot, it is derived here from the description of Reuss and Humboldt; and a worse piece of geological observation and reasoning, we will venture to say, was never printed. That it is a fresh water formation appears almost certain, so far from being what is represented; and that the observers have confounded and misrepresented the relative positions of the fresh water and marine strata, is equally so, though we cannot here enter into the reasons for this opinion.

As to the ichthyology, it is of a piece with the geology, which is less pardonable, since the author's claims in this department are more decided. The figure is that of a sturgeon, and yet he chooses to decide that it is an unknown pike, to which he gives the name of Esox acutirostris. Stromatæus and Pæcilia, from the same place, are determined on grounds as slender. The specific name of the latter is Dubia; and if we were inclined to make a very low jest, we should say that it was applicable to three fourths of the whole collection.

Mons. Blainville has entered into considerable length on the subject of the celebrated fishes of Monte Bolca, and we are glad to say that in this part of his treatise he has been of real service to the cause in hand. It is, in truth, the most valuable, and we had almost said the only valuable part of his book. With the double advantage of the splendid work, published at Verona under Volta's direction, and of the collection itself procured, (plundered, as Italy asserts,) from Count Gazzola, he has been enabled to rectify the more glaring errors of the Italian naturalists, and to give somewhat like a rational list of the specimens. Out of Volta's forty-four genera, including a hundred and five species, he has admitted only ninety-three species, and it is quite plain that they would allow of still further purification.

This subject, however, is so extensive that we dare not enter it. We have no room for a criticism on genera and species, which indeed could not be rendered intelligible without the figures. But we are bound to say, on the geological question, that the Italian theory which collects these fishes from all parts of the world, is purely gratuitous; and thus while it is geologically impossible and groundless, it is contradicted icthyologically by the specimens themselves, which are now, in part, and in former times have probably all been, natives and residents of that sea which now washes the land in which their

remains are preserved. It would be easy to state a rational theory of this formation, and its relations to parallel phenomena in many parts of the world: but they are in a good measure superseded by the present more rational views of the history of the supramarine organiferous strata.

We said before, that it was incumbent on an author, professing to give a general treatise on ichthyolites, or of any other branch of this science, to make himself acquainted with the facts at least which are accessible, or to acquire such knowledge as would enable him to profit by the observations of others. The defect of our author in these respects is peculiarly sensible in what follows, where he has copied careless observations in a careless manner, and rendered the confusion more offensive and troublesome by the systematical and decided form in which he has placed it. Mount Lebanon, Cerigo, Antibes, and many other localities are discussed in this slovenly manner, from Faujas de St. Fond, and others; and where we ought to have certainty we have only useless guessing.

We consider that an author who thus professes to write a systematical work, is bound to make it really systematical, as far as that is possible. It is a different thing to write single essays, or to describe those separate localities and partial facts from which geology ultimately derives assistance towards its general views. Hence our author is equally deserving of censure, that when he quotes Sicily, Malta, Iceland, and other well known countries, where fossil fishes have been found, he is scarcely ever at the trouble of ascertaining what has already been written about them, or of trying to extract something like truth from a balance of testimonies. As a specimen of this unpardonable carelessness, he quotes Antibes as a locality, and then doubts whether it is not Antigua. Nor could any thing but the same ambition to make a book and a system, which has led him to give genera without descriptions and species, under such imaginary and nominal genera, have tempted him to muster in his arrangement the fossil fishes of China, of which he knows nothing. On those of England he is equally unsuccessful; whereas he might have found something to his purpose, had he taken the trouble to seek for it.

We have already shown our suspicions that many of Mons. Blainville's marine formations, and marine ichthyolites, are really fresh water examples; but we have a detail in the latter part of his essay of those which are indisputably such, and which he chooses to call Potamiens, "apparemment," because Ποταμος is a river, and that these strata have been produced in lakes.

We may pass over the Italian examples, as unsatisfactory: those of France are better known, and are here better described. The deposit of Aix is well known to consist of five marked beds, reaching to a depth of near sixty feet, consisting of marl, limestone, bituminous marl slate, and gypsum. The fishes which it contains are one species in the genus perca, a cyprinus, and the mugil cephalus or

grey mullet. This latter is a sea fish, and an inhabitant of the Mediterranean; though, as we formerly remarked, it can live in fresh water; and since this is a decided fresh water formation, here is a remarkable fact in proof of what we have already advanced on the uncertainty which attends this subject, and which, if Mons. Blainville had not been so strenuous a theorist, might have led him to be more cautious in many of his decisions on this subject.

The basin of Paris has been so thoroughly described by Cuvier and Brongniart, that little has been left for our author to do; and that description is also known to every one sufficiently, to render it unnecessary for us to enter into any details respecting it. These specimens are not numerous, and they are generally very imperfect and ill preserved. As our geological readers must know, they have been described by Lamanon, Faujas de St. Fond, and de la Metheric, as well as by Cuvier. The species are limited to seven, and they are all so ill defined that no very satisfactory conjecture respecting them has yet been made. We shall not quote what has been said, as it is of no moment in the present review of Mons. Blainville.

Yet we must be indulged in one remark on Cuvier himself in this case; professing, at the same time, that respect for his attainments which it is almost superfluous to profess. When the fishes of Vestenu nuova were first described, it was the fashion to suppose that the world had been turned upside down, and inside out, and if there were two ways of explaining a fact, it seemed to be the fashion and the ambition to reject the easiest and most natural solution, for the sake of adopting what was marvellous, incredible, or impossible. This has indeed been one of the leading diseases of geology and geologists.—Because it was impossible that obsidian and pumice could be formed by water, they were to be aqueous productions: because the identity of volcanic rocks and trap rocks was so absolute that we could almost suppose we had seen the latter formed by the same class of fires which produced the former, it was resolved that they were generated from water. Thus, at Vestenu nuova, because there was no difficulty whatever that the crowd of fishes which inhabit, or inhabited, the Mediterranean should have been elevated from the bottom of the sea, entangled in its mud, and indurated in rock, just as they have been before our very eyes in Iceland, it became necessary to collect them from the four quarters of the globe. The simple solution was not marvellous enough, and the dreams of Volta and his party have been triumphantly repeated and re-echoed, in our own clearer day, by those who prefer doubt and difficulty, to conviction and facility, and would rather that truth should not be attained than attain it by the easy road which all may apprehend.

Of this, we fear, we must accuse Cuvier himself in the case of Pœcilia vivipara, (as he chooses to suppose it,) of the Paris basin, a fish figured by Bloch, and a native of Surinam. For what possible purpose should we resort to Surinam for a fish for this situation? The

theory is as purposeless as the voyage of the living fish itself to th Seine would be at this day, unless his object were to attract the praises, in a Bechamel sauce, of the Gastronomes who sit in judgment at the tables of Beauvilliers. The very theory of the Paris basin, to speak seriously, renders this supposition nearly as impossible as it is unreasonable, and it would surely be a more rational conclusion that the fish in question was either a lost native of the Parisian seas, or that the imperfection of the specimen was the cause of a resemblance far too slight and doubtful to give the slightest justification to such a useless and violent supposition.

We are fully aware at the same time of the argument in favour of such a view, which may be founded on the existence of vegetables with intertropical characters or analogies, in the same climates in which the Paris basin is situated. But this whole question, as far as it relates to change of climate, or an alteration in the position of the earth's axis, is very obscure, or more than obscure: and were it not so, it must be remembered that the coal strata belong to a far remote period of the earth, antecedent by many and by millenarian revolutions to the basin and deposit in question. We have no right to argue thus, and it is only to perpetuate the vice from which geology has already suffered so severely.

But it is time for us to bring this article to a close, and to take our leave of Mons. Blainville and his ichthyolites. We wish that we could have spoken more favourably of a performance which contains far more of conjecture and trifling than of useful and solid information, and which is not calculated to add much to our stock of knowledge. We do not undervalue this particular pursuit; on the contrary, we think it highly desirable that every organic fragment of a former world, in every department, should be collected, studied, and described: but geology, geology itself, the history of the structure and revolutions of the earth, has also its claims; and such collections and systems more than double their value when they are caused to bear and throw light on this important subject. This is what Mons. Blainville has yet to learn. We still hope and expect that he will look at his subject in this view; that he will turn from the poor ambition of shining in a catalogue of new and useless names, to that of improving the sciences which he has undertaken; and that, substituting study for guessing, and close investigation and careful reasoning for compilation and catalogue, he will appear before us again, at some future day, a new man, to receive the praise which we shall give with far more pleasure than we have passed the censure.

II. *The Philosophical Transactions of the Royal Society of London, for the year* 1823. PART. II.

1. *On a New Phenomenon of Electro-Magnetism.* *By* Sir Humphry Davy, Bart. Pres. R.S.

This is a contribution of a curious fact to the new and interesting science of electro-magnetism, and it is by such contributions alone that this infant science can, at present, be expected to make any progress to maturity. Sir H. Davy found, that when two wires were placed in a basin of mercury, perpendicular to the surface, and in the voltaic circuit of a battery with large plates, and the pole of a powerful magnet held either above or below the wires, the mercury immediately began to revolve round the wire as an axis, according to the circumstances of electro-magnetic rotation, discovered by Mr. Faraday. Masses of mercury, of several inches in diameter, were set in motion, and made to revolve in this manner whenever the pole of the magnet was held near the perpendicular of the wire; but when the pole was held above the mercury, between the two wires, the circular motion ceased, and currents took place in the mercury in opposite directions, one to the right and the other to the left of the magnet. Other circumstances led to the belief that the passage of the electricity produced motions independent of the action of the magnet, and that the appearances were owing to a composition of forces.

The form of the last experiment was inverted, by passing two copper wires through two holes, three inches apart, in the bottom of a glass basin; the basin was then filled with mercury, which stood about the tenth of an inch above the wire. Upon making a communication through this arrangement, with a powerful voltaic circuit, the mercury was immediately seen in violent agitation; its surface became elevated into a small cone above each of the wires; waves flowed off in all directions from these cones, and the only point of rest was apparently where they met in the centre of the mercury, between the two wires. On holding the pole of a powerful magnet at a considerable distance above one of the cones, its apex was diminished and its base extended. At a smaller distance, the surface of the mercury became plane, and rotation slowly began round the wire. As the magnet approached, the rotation became more rapid; and when it was about half an inch above the mercury, a great depression of it was observed above the wire, and a vortex which reached almost to the surface of the wire.

The President thinks that these phenomena are not produced by any changes of temperature, or by common electrical repulsion, and concludes that they are of a novel kind.

2. *On Fluid Chlorine.* *By* M. Faraday, Chemical Assistant in the Royal Institution.

[Communicated by Sir H. Davy, Bart., Pres. R.S.]

This paper describes Mr. Faraday's first step in the important series of experiments, which led to the condensation of the gases. He prepared some dry hydrate of chlorine, at a low temperature, and introduced it into a glass tube, which was hermetically closed. Being placed in water at 100, the substance fused, the tube became filled with a bright yellow atmosphere, and on examination was found to contain two fluid substances: the one was of a faint yellow colour, and the other a heavy bright yellow fluid, lying at the bottom of the former, without any apparant tendency to mix with it. This fluid was easily distilled in a bent tube, and separated from the former. When the whole was allowed to cool, neither of the fluids solidified at a temperature above 34°, and the yellow portion not even at 0°. When the two were mixed together, they gradually combined at temperatures below 60°, and formed the same substance as that at first introduced. If, when the fluids were separated, the tube was cut in the middle, the parts flew asunder with an explosion, the whole of the yellow portion disappeared, and there was a powerful atmosphere of chlorine produced. The pale portion remained, and proved to be a weak solution of chlorine in water.

The result of this experiment was confirmed by condensing perfectly dry chlorine by a syringe, and then exposing it to a low temperature; it was thus readily made to assume the liquid form.

Fluid chlorine appears very limpid and fluid, and is excessively volatile at common pressure. Upon cooling a portion to 0° and then opening the tube, a part immediately flew off, leaving the rest so cooled by evaporation as to remain a fluid under the atmospheric pressure. Mr. Faraday thinks that the temperature could not have been above −40° in this case.

He calculates the specific gravity of fluid chlorine at 1·33.

In a note to this paper, the President of the Royal Society shews that these results will evidently lead to other researches of the same kind, and mentions, that by sealing muriate of ammonia and sulphuric acid in a glass tube, and causing them to act upon each other, he had procured liquid muriatic acid.

3. *On the Motions of the Eye, in Illustration of the Uses of the Muscles and Nerves of the Orbit.* *By* Charles Bell, Esq.

[Communicated by Sir H. Davy, Bart., P.R.S.]

This is a highly interesting paper, and, together with the second part, inserted in another part of the volume, is calculated to ex-

plain many ill-understood points of the mechanism and functions of the eye, and to renew our wonder at the properties of the organ itself, and the frame-work and apparatus by which it is suspended, moved, and protected. Mr. Bell concludes from his researches, that the high endowments which belong to this wonderful structure depend not exclusively, as is generally conceived, upon the ball and optic nerve, but upon its exterior apparatus also. It is to the muscles, and to the conclusions, we are enabled to draw from the consciousness of muscular effort, that we owe that geometrical sense by which we become acquainted with the form and magnitude and distance of objects. It is impossible to do justice to Mr. Bell's views in the short space to which we are obliged to confine ourselves in these abstracts: a careful perusal of the whole paper is absolutely necessary to those who would wish thoroughly to understand the investigation, and will amply repay even the more general reader. The author has shewn, by the most satisfactory illustrations, that we must distinguish the motions of the eye according to their objects or uses, whether for the direct purpose of vision, or for the preservation of the organ; that the eye undergoes a revolving motion not hitherto noticed; that it is subject to a state of rest and activity; and that the different conditions of the retina are accompanied by appropriate conditions of the surrounding muscles; that these muscles are to be distinguished into two natural classes; and that in sleep, faintness, and insensibility the eye-ball is given up to the one, and in watchfulness and the full exercise of the organ, it is given up to the influence of the other class of muscles; and, finally, that the consideration of these natural conditions of the eye explains its changes as symptomatic of disease, or as expressive of passion.

4. *An Account of an Apparatus on a peculiar Construction for performing Electro-Magnetic Experiments. By* W. H. Pepys, Esq., F.R.S.

5. *On the Condensation of several Gases into Liquids. By* M. Faraday, Chemical Assistant in the Royal Institution.

[Communicated by Sir H. Davy, Bart., P.R.S.]

In this paper, Mr. Faraday follows up the train of investigation which the condensation of chlorine, by its own elastic power, so obviously opened. Mercury and sulphuric acid were sealed up in a bent tube, and being brought to one end, heat was applied, whilst the other end was preserved cool by wet paper. The sulphurous acid, which was generated, passed to the cold end, and was condensed into a liquid. The properties of liquid sulphurous acid are as follow:—It is limpid and colourless; its refractive

power about equal to that of water; it does not congeal at a temperature of 0°; its specific gravity is nearly 1·42, and it exerts a pressure of about two atmospheres at 45°.

Sulphuretted hydrogen was generated and condensed in an analogous way, from muriatic acid and sulphuret of iron. It was colourless, limpid, and excessively fluid. It was not rendered more adhesive by a temperature of 0°; its refractive power appeared to be rather greater than that of water, and the pressure of its atmosphere at 50, was equal to about 17 atmospheres. Its specific gravity about 0·9.

Carbonic acid was also condensed, but it required great precautions to effect the condensation with safety. It is a limpid, colourless body, extremely fluid, and floated, as did all the preceeding liquids, upon the contents of the tube, without mixing. It distils readily at the difference of temperature between 32° and 0°; its refractive power much less than that of water, and its vapour exerts a pressure of 36 atmospheres at a temperature of 32°. In endeavouring to open the tubes which contained it at one end, Mr. Faraday states, that they uniformly burst with powerful explosions.

Fluid euchlorine was also obtained, and proved to be a transparent substance, of a deep yellow colour, and highly elastic powers.

Liquid nitrous oxide is limpid and colourless. It boils rapidly by the difference of temperature between 50 and 0°, and does not solidify at – 10. Its refractive power is less than that of any known fluid, and the pressure of its vapour is equal to above fifty atmospheres at 45°.

Liquid cyanogen is limpid, colourless, and very fluid, and does not alter its state at the temperature of 0°. Its refractive powers rather less than that of water; its specific gravity nearly 0·9, and the pressure of its vapour about 3·7 atmospheres.

Mr. Faraday obtained dry ammonia from chloride of silver saturated with this gas*, and, by the usual process, succeeded in condensing it. It was colourless, transparent, and very fluid. Its refractive power surpassed that of water, and all the other liquids hitherto described. The pressure of its vapour is equal to about 6·5 atmospheres at 50°, and its specific gravity is 0·76. Attempts were made to obtain hydrogen, oxygen, fluoboracic, fluosilicic, and phosphuretted hydrogen gases in the liquid state; but though all of them have been subjected to great pressure, they have as yet resisted condensation.

6. *On the Application of Liquids formed by the Condensation of Gases as Mechanical Agents. By* Sir Humphry Davy, Bt. Pres. R.S.

In this paper Sir H. Davy anticipates the probability of the ap-

* See Quarterly Journal, vol. v. p. 74.

plication of the elastic force of compressed gases to the movement of machines. He founds this anticipation upon the immense differences between the increase of elastic force in gases under high and low temperatures, by similar increments of temperature. The force of carbonic acid was found to be equal to that of air compressed to $\frac{1}{20}$ at 12°, and of air compressed, to $\frac{1}{33}$ at 32°, making an increase equal to the weight of thirteen atmospheres, by an increase of 20° of temperature.

On the Temperature at considerable depths of the Caribbean Sea. By Captain Edward Sabine, F.R.S.

[In a Letter addressed to Sir H. Davy, Bart., P.R.S.]

Captain Sabine found the temperature of the water, at a depth of 6000 feet, in latitude 20¼ N. and long. 83¼ W. near the junction of the Mexican and Caribbean Seas, to be 45°.5, that of the surface being 83°. He infers, that one or two hundred fathoms more line, would have caused the thermometer to descend into water at its maximum of density as depends on heat; this inference being on the presumption that the greatest density of salt water occurs, as is the case in fresh water, at several degrees above its freezing point.

8. *Letter from* Captain Basil Hall, R.N., *to* Captain Kater, *communicating the details of Experiments made by him and* Mr. Henry Foster, *with an Invariable Pendulum; in London; at the Galapagos Islands in the Pacific Ocean, near the Equator; at San Blas de California on the N.W. Coast of Mexico; and at Rio de Janeiro in Brazil. With an Appendix, containing the Second Series of Experiments in London, on the Return.*

The title is an abstract of the paper, and the following are the most exact results obtained by Captain Hall at each station.

Stations.	Diminution of Gravity From Pole to Equator	Ellipticity.	Length of Equat. Pend.
Galapagos, 0° 32′ ″ N	.0051412	$\frac{1}{284.98}$	39.017196
San Blas, 21 30 25 N	.0054611	$\frac{1}{313.55}$	39.00904
Rio, . . . 22 55 22 S	.0053431	$\frac{1}{302.37}$	39.01206

9. *Second Part of the Paper on the Nerves of the Orbit.* *By* Chas. Bell, Esq.

[Communicated by Sir H. Davy, Bart., P.R.S.]

This is a continuation of the subject upon which Mr. Bell had entered in his last paper. His object is to explain the reason of there being six nerves distributed to the eye, and crowded into the narrow space of the orbit. In this investigation he demonstrates, that there is a correspondence between the compound functions of an organ and the nerves transmitted to it. It is impossible to do more, than here sum up the distinct functions of the nerves, as unravelled by the skill of the author.

"The first nerve is provided with a sensibility to effluvia, and is properly called the olfactory nerve.

"The second is the optic nerve, and all impressions upon it excite only sensations of light.

"The third nerve goes to the muscles of the eye solely, and is a voluntary nerve by which the eye is directed to objects.

"The fourth nerve performs the insensible traversing motions of the eyeball. It combines the motions of the eyeball and eyelids, and connects the eye with the respiratory system.

"The fifth is the universal nerve of sensation to the head and face, to the skin, to the surfaces of the eye, the cavities of the nose, the mouth and tongue.

"The sixth nerve is a muscular and voluntary nerve of the eye.

"The seventh is the auditory nerve, and the division of it, called *portio dura*, is the motor nerve of the face and eyelids, and the respiratory nerve, and that on which the expression of the face depends.

"The eighth, and the accessory nerve, are respiratory nerves.

"The ninth nerve is the motor of the tongue.

"The tenth is the first of the spinal nerves; it has a double root and a double office; it is both a muscular and a sensitive nerve."

Mr. Bell concludes his paper with a few very appropriate words in favour of anatomy, as a means better adapted for discovery than experiment.

"Anatomy," he observes, "is already looked upon with prejudice by the thoughtless and ignorant: let not its professors unnecessarily incur the censures of the humane. Experiments have never been the means of discovery; and a survey of what has been attempted of late years in physiology will prove; that the opening of living animals has done more to perpetuate error, than to confirm the just views taken from the study of anatomy and natural motion."

With another opinion of Mr. Bell's we cannot also but coincide, and that is, that "Medical histories do not often lead to the improvement of strict science."

It is an opinion worthy the consideration of the Committee of Papers of the Royal Society.

10. *An Account of Experiments made with an Invariable Pendulum at New South Wales.* *By* Major-General Sir Thomas Brisbane, K.C.B. F.R.S.

[Communicated by Captain HENRY KATER, F.R.S., in a Letter addressed to Sir H. DAVY, Bart., P.R.S.]

The results of Sir Thomas Brisbane's experiments are as follow: 39·07696 inches the length of the pendulum, vibrating seconds at Paramatta; .0052704 the diminution of gravity from the pole to the equator, and $\frac{1}{295,84}$ the resulting compression.

11. *Observations and Experiments on the daily Variation of the Horizontal and Dipping Needles under a reduced directive Power.* *By* Peter Barlow, Esq., F.R.S., of the Royal Military Academy.

[Communicated by DAVIES GILBERT, Esq., V.P.R.S.]

The daily change of the horizontal needle is so small, that it has only hitherto been detected with the most careful observations, and with the most delicate instruments; and in the dipping needle, that change is so extremely minute, as to have escaped observation altogether. It occurred to Mr. Barlow, that it would be possible to increase this deviation in both needles, so as to render it distinctly observable, by reducing the directive power of the needle, by means of one or two magnets properly disposed, to mask, at least in part, the terrestrial influence. This idea was realized, and in this way it is easy to produce a daily variation, to almost any amount. From his experiments, Mr. Barlow draws the following conclusions:—

1st. That while the north end of the needle is directed to any point from the south to N.N.W. its motion during the forenoon is towards the left hand, advancing to some point between the N.N.W. and north; and while it is directed towards any point between the north and S.S.E., it passes to the right hand, that is, still to some point between the north and N.N.W.

2dly. That the daily change is not produced by a general deflection of the directive power of the earth, but by an increase and decrease of attraction, of some point situated between the north and N.N.W., or between the south and S.S.E.

3dly. That the dipping needle is subject to a daily variation, which cannot, at present, be reduced to any fixed principles.

12. *On the Diurnal Deviations of the Horizontal Needle when under the influence of Magnets.* *By* Samuel Hunter Christie, Esq., M.A., Fellow of the Cambridge Philosophical Society: of the Royal Military Academy.

[Communicated by Sir H. DAVY, Bart., P.R.S.]

Mr. Barlow communicated to Mr. Christie his method of ren-

dering the variations of the magnetic needles more sensible, and he commenced a series of observations in consequence of the communication.

He ascertained, that there was an easterly deviation before eight o'clock in the morning, and that the greatest westerly deviation took place about one o'clock in the afternoon. He also found, that the state of the weather had a considerable influence upon the nature and extent of the changes. But the most striking effects seemed to him to arise from changes of temperature, and he adopts the opinion that temperature, if not the only cause of the daily variation, is the principal. He expresses his intention of entering fully into the general question, when he shall have ascertained the precise effects of changes in the temperature of magnets.

13. *On Fossil Shells. By* Lewis Weston Dillwyn, Esq., F.R.S.

[In a Letter addressed to Sir H. Davy, Bart., P.R.S.]

Mr. Dillwyn remarks, that every turbinated univalve of the older beds, from transition lime to the lias, of which he can find any record, belongs to the herbivorous genera, and that the family has been handed down through all the successive strata, and still inhabits our land and waters. On the other hand, all the carnivoruos genera abound in the strata above the chalk, but are comparatively extremely rare in the secondary strata, and not a single shell has been detected in any lower bed than the lower oolite. He thinks, that a further examination will prove, that neither the aporrhaides or any of those few undoubtedly carnivorous species, which have been found in the secondary formations, were furnished with predaceous powers, but that they belong to a subdivision of the trachelipoda zoophaga, which feed only on dead animals.

14. *On the apparent Magnetism of Metallic Titanium. By* William Hyde Wollaston, M.D., V.P.R.S.

In this paper Dr. Wollaston corrects an oversight in his former communication upon metallic titanium. He therein stated, that when the crystals from the slag had been freed from all particles of iron adherent to them, they appeared to be no longer acted upon by the magnet. He has since found, that although they are not sufficiently attractive to be wholly supported by the magnet, yet, when a crystal is supported by a thread, the force of attraction is sufficient to draw it twenty degrees from the perpendicular. From an ingenious comparison of different magnetic forces, he calculates that $\frac{1}{250}$ part of iron, as an alloy in the metallic titanium, would be sufficient to account for this power; and he shews, that it is extremely difficult chemically to detect so minute a portion of iron, on account of the high colour of the precipitates of titanium.

15. *An Account of the Effect of Mercurial Vapours on the Crew of His Majesty's Ship Triumph, in the year* 1810. *By* William Burnett, M.D., *one of the Medical Commissioners of the Navy, formerly Physician and Inspector of Hospitals to the Mediterranean Fleet. Communicated by* Matthew Baillie, M.D., F.R.S.

The particulars of this curious case have been already published by Dr. Baird, in *Nicholson's Journal*, for the month of Oct. 1810.

16. *On the Astronomical Refractions. By* J. Ivory, A.M., F.R.S.

This is a very long and laborious investigation of the problem of astronomical refraction; its result is a new table of refractions with which the paper concludes, and which is compared with other tables that have been long in the hands of astronomers, and the characters of which are well established. Mr. Ivory shews that it is fruitless to expect a near agreement in every single instance between observation and any table of refractions whatever, and that there is no test of their accuracy except the smallness of the mean error in a series of observations made at different times.

17. *Observations on Air found in the Pleura, in a case of Pneumatothorax; with Experiments on the Absorption of different kinds of air introduced into the pleura. By* John Davy, M.D., F.R.S.

This is a medical history which Dr. Davy has endeavoured to illustrate by some experiments upon dogs. He observes that the circumstances which he has ventured to bring forward are somewhat favourable to the idea of the secretion or exhalation of azote, but are still far from conclusive.

18. *On Bitumen in Stones. By* the Right Honourable George Knox, F.R.S.

This is a second paper upon the same subject. Mr. Knox finds bitumen in every thing except rock crystal and pearl-white adularia.

19. *On certain Changes which appear to have taken place in the Positions of some of the principal fixed Stars. By* John Pond, Astronomer Royal, F.R.S.

The Astronomer Royal thinks that his observations lead to the conclusion that some variation, either continued or periodical, takes place in the sidereal system, which producing but very small deviations in a finite portion of time, has hitherto escaped notice. The nature of this motion appears to be such that the stars are now mostly found a considerable quantity to the southward of their computed planes. With respect to the laws by which these motions are governed, the observations in question, he admits, are not sufficiently exact to throw any light upon them.

[To the Editor of the QUARTERLY JOURNAL OF SCIENCE, &c.]

Manchester, March 1, 1824.

SIR,

THE review of the 9th edition of my Elements of Chemistry, in the last number of your Journal, contains some animadversions, to which I trust you will do me the justice to insert a brief reply. It is not, indeed, my intention to follow the reviewer through the variety of topics which he has introduced, but to confine myself to a few of those, on which I am most desirous to be set right with your readers, and which involve questions of some importance to chemical philosophy.

It has happened unfortunately that a passage, expressing doubts of the correctness of the theory of volumes, which certainly ought to have been expunged from the present edition of my work, was overlooked, owing to one or two of the early sheets having been revised under circumstances disadvantageous to correctness. For this oversight, I am content to take upon myself whatever blame it may justly deserve; and I should have had no reason to complain, had the reviewer pointed out the striking inconsistency of the passage, which he has quoted, with other parts of my volumes. At page 299, vol. i., for example, I state, "analogy is certainly in favour of this opinion, for the instances are numerous in which gaseous bodies observe the law respecting volumes deduced by Gay-Lussac, and we have not at present any well-ascertained exception to it." The tenor of the whole work, also, is inconsistent with the rejection of the theory of volumes imputed to me by the reviewer; for almost every chapter affords examples of compounds constituted in conformity to the law; and at the close of the second volume I have inserted, for the first time, a table exhibiting a general view of such compounds.

The reviewer complains (p. 338,) that I have not given a more elaborate and consistent account of the atomic theory, though he represents it (p. 339) as requiring "mystifications," and particularly marks the distinction between the atomic *hypothesis* and the *theory* of volumes. To a certain extent, the law of volumes is, I admit, the expression of a general fact, of which we have the indubitable testimony of our senses. But with regard to certain elementary substances, which are not known to us separately in a gaseous state, it is entirely matter of inference that *their* vapours unite in volumes, which are either equal, or multiples or sub-multiples of each other. We have, for example, no argument but from analogy, that this holds with respect to carbon; nor, if we admit the probability of such combinations, have we any decisive proof that the volumes, which have been assigned, are actually the true ones. In all such cases, where we have not access to the facts by direct experiment, the law of volumes rests on the ground of analogy only; and is so far purely theoretical. The law, also, however well established with respect to gaseous bodies, is limited to them only; and we must seek for some other principle, to explain the far greater number of chemical combinations which take place between bodies existing under other forms.

In the investigations which have led Mr. Dalton to the atomic system, it appears to me that he has pursued no other method of reasoning than that which has been followed by the most successful cultivators of natural science, since the introduction of the inductive logic. The theory of

gravitation itself, however firmly it may now be established, took its rise in an hypothesis founded on analogy, and could be considered as nothing more than an hypothesis, till that period of the life of its great author, when the coincidence was ascertained between the law which regulates the fall of heavy bodies, and that power which preserves the moon in her orbit. "A principle," it has been remarked by the late Professor Playfair, "is often admitted in physics, merely because it explains a great number of appearances, and the theory of gravitation itself rests on no other foundation*." The term hypothesis, then, is far from being one of just reproach, since it may be applied in a variety of cases to those first steps which it has been found necessary to take in philosophical inquiries, and which have led eventually to well-established laws.

The views of Mr. Dalton respecting the atomic constitution of bodies appear to me to be founded mainly on the general fact, *that bodies unite in definite proportions.* Of this general truth, Richter certainly furnished the best and fullest evidences. Far from wishing to "suppress" the share of credit to which he is entitled, I have alluded to the table, calculated by Fischer from his experiments; but it is omitted in the appendix to the present edition, merely because it has been superseded by the more extensive tables of equivalents, which have since been constructed. The law of combination *in multiple proportions*, the first experimental proofs of which are due to Mr. Dalton, comes strongly in aid of the atomic theory, and furnishes its most striking proofs and illustrations. Nothing can be more evident than that if we set out from a binary compound, whose gaseous elements exist in equal volumes, and proceed to compounds of the same elements, in which either is found as a multiple in volume of the other, there must, as the reviewer observes, "be a perfect accordance between the atomic hypothesis and the theory of volumes." But the atomic theory is, I contend, a wider and more comprehensive generalization, and includes the law of volumes as well as that of combination by multiples of weights. In this case, as in many others, when we advance from discovery to discovery, we do nothing more than resolve our former conclusions into others still more general.

There can surely be nothing inconsistent with sound philosophy in inquiring *why* bodies unite in definite proportions, and *why* they unite in proportions which are multiples or sub-multiples of weights or of volumes; and the only satisfactory explanation, that has yet been given of these facts is, that in those combining weights, which are represented by equivalent numbers, are contained determinate numbers of ultimate particles or atoms, and that from the relative weights of aggregates that combine, we may deduce the proportions as to weight which the ultimate single atoms bear to each other. As there seems every reason to believe that chemical attraction is exerted, not between masses, but between ultimate particles or atoms only, combination will then take place either between single atoms or when either is in excess, the excess will be represented by some simple multiple of the number of atoms. In this reasoning it is of course taken for granted that matter is not infinitely divisible, a position rendered extremely probable by a philosopher, to whose opinions the reviewer will agree with me in paying the greatest deference. "Now though we have not the means," that writer observes, "of ascertaining the extent of our own atmosphere, those of other planetary bodies are nevertheless objects for astronomical investi-

* Playfair's Works, vol. iv. p. 62, note.

gation; and it may be deserving of consideration, whether, in any instance, a deficiency of such matter can be proved, and whether, from this source, any conclusive argument can be drawn in favour of ultimate atoms of matter in general. For since the law of definite proportions, discovered by chemists, is the same for all kinds of matter, whether solid, fluid, or elastic, if it can be ascertained that any one body consists of particles no longer divisible, we can then scarcely doubt that all other bodies are similarly constituted; and we may, without hesitation, conclude that those equivalent quantities, which we have learned to appreciate by proportionate numbers, do really express the relative weights of elementary atoms, the ultimate objects of chemical research*." A body so constituted (it is the scope of the essay which has been just quoted to shew) is found in the earth's atmosphere, all the phenomema according with the supposition that it is "of finite extent, limited by the weight of ultimate atoms of definite magnitude, no longer divisible by repulsion of their parts."

But though the atomic theory, in its general outline, seems to me to rest sufficiently on the evidence of facts, and on legitimate reasoning, yet there are some positions which have arisen out of it, that may or may not be true, without, in the latter case, impeaching its general correctness. Of this nature are the two cited by the reviewer, (p. 340) especially the first, *viz.*, "that an increase of the density of a gas indicates an increased number of simple atoms associated in the compound atom." This principle, I am ready to admit, may have been too hastily deduced; for besides that it is at variance with the view which I have adopted of the nitrous compounds, it is inconsistent also with that which I have taken of the compounds of carbon and hydrogen; olefiant gas, the binary compound, being denser than light carburetted hydrogen, the ternary one. The other position, that "of chemical compounds the most simple, is the most difficult to be decomposed," stands unimpeached, and is exemplified, as the reviewer himself remarks, in the greater difficulty of decomposing nitrous oxide than nitrous gas. To Mr. Dalton's opinion of nitrous gas, which makes it the binary compound, its greater facility of decomposition might present a reasonable objection. But it is quite inconsistent with sound reasoning to frame a proposition out of Mr. Dalton's views and mine, which are completely at variance as to the compounds of nitrogen, and to apply to that proposition the syllogistic method of reasoning as a test of its truth. No syllogism can be so constructed as to involve in the same dilemma two persons, who disagree with each other as to the conditional proposition on which that syllogism is founded.

Though I have adopted, as most probable, that view of the nitrous compounds which makes the elements of nitrous oxide to exist in binary and those of nitrous gas in ternary, atomic proportion, yet I consider the truth of this opinion as far from being demonstrated That the volumes of the elements of those two compounds are what they nave been represented by Gay-Lussac, I entertain very little doubt, not only from the evidence of other persons, but from methods of analysis which I have myself devised, and which, though not otherwise important, than as they bring out the results by easy and summary processes, I shall probably ere long lay before the public. But it must still remain a subject of inquiry, whether equal volumes of nitrogen and oxygen gases contain, as Mr. Dalton supposes, equal numbers of atoms; or whether, as I take to be more probable, the same number of atoms exists in one volume of oxygen as in two of nitrogen gas.

* Dr. Wollaston on the Finite Extent of the Atmosphere.—*Phil. Trans.* 1822.

The illustrious author of the *Elements of Chemical Philosophy* will not, I trust, require any assurance from me, that nothing could be farther from my design, or more repugnant to my feelings, than to misunderstand "intentionally" his ideas respecting chemical combination. I have, it is true, rendered the word *proportion* by that of *atom*, but I have enclosed the latter word in parentheses, purposely to shew that it was not the expression of the author, but my own interpretation of his meaning. The fact is, that great ambiguity has arisen out of the use which has been made of the word *proportion*. Strictly, the only numerical expressions of proportion, that can be considered as "the results of experiment," must be derived from a comparison either of the weights, or of the volumes, in which bodies unite; and it appears to me that a system of numbers, derived from the consideration of weights, should be kept distinct from one derived from a comparison of volumes. But the numbers (1 and 15) representing hydrogen and oxygen, were gained from the joint consideration of the *weight and volume* of the elements of water; while those representing oxygen and nitrogen (15 and 26) were derived from a comparison of the *weights only* of the elements of nitrous oxide. Since, then, the word proportion could not, in both cases, apply to a comparison of weights only, nor yet of volumes only, it was natural for me to conclude that it must bear a reference to ultimate particles or atoms, the only other objects, which I could conceive as, in this case, admitting of being compared.

These, Sir, are the only points respecting which I deem it necessary to trespass on the attention of your readers, though there are others on which I am not disposed to concede the justice of the reviewer's strictures. In some instances, I allow, he has pointed out mistakes that may call for correction on a future occasion, should any occur to me. Having invited the communication of errors or omissions, with a view to the improvement of my volumes, it would ill become me to feel "offended" when that invitation is complied with; and all that I claim is to be animadverted upon with a reasonable share of courtesy and of candour.

I am, Sir,

Your obedient and faithful servant,

WILLIAM HENRY.

ART. XIII. PROGRESS OF FOREIGN SCIENCE.

IN Volume XIII. p. 144, we briefly animadverted on some researches of Professor Gmelin, of Tubingen, published in Dr. Brewster's Journal, about two years ago, where the terms refute, and refutation were applied with more freedom, than propriety or decorum, to Sir. H. Davy's fundamental experiments relative to the connexion of chemical affinity with electrical attractions, contained in his Bakerian Lecture of 1806. M. Becquerel read to the Academy of Sciences on the 7th June, 1823, an interesting paper on the electrical effects which are developed during different chemical actions, which perfectly accord with, and seem fully to confirm, the conclusions of the English philosopher. After a candid retrospect of preceding inquiries on the subject, M. Becquerel thus states Sir H. Davy's theory: "Supposing two bodies, whose molecules are in different states of electricity, and that these states are sufficiently exalted to give them an attractive force, superior to the power of aggregation, a combination will be formed. This is the key of the electro-chemical theory." "Although Sir H. Davy has advanced the opinion, that the substances which combine are those which manifest on mutual contact, opposite electrical states, yet we perceive from his own experiments that it is by induction he extended this property to all the bodies which exert chemical actions on one another; for instance, he was not able to verify it on alkaline and acid substances, unless they were perfectly dry. In other cases, the results were null. He adduces, among others, pure potash, and sulphuric acid, which afford no appearance of electricity at the moment of their combination. In fact, this celebrated chemist could not recognise electricity in the contact of two substances which are just combining; for, adopting the electro-chemical theory, as soon as the combination takes place, the two electricities that were developed, recombine, and probably form, by their union, caloric: whence, in making use of a condenser to collect one of the electricities which is disengaged, traces of this fluid ought to be found with difficulty, since the condenser requires a certain time to charge itself, during which the two electricities may re-combine. But if a galvanometrical multiplier be employed, such as that of M. Schweigger, which renders the electricities sensible at the very instant of their disengagement, and consequently at the instant when the combination takes place, currents will be obtained of greater or less force, according to the degree of conductibility of the substances put in action, and that of their reciprocal affinities; I say according to the degree of conductibility, because when one of these substances conducts the

electricity ill, there is no current, although the chemical action be very strong. The conductibility then is here an indispensable condition.

We shall examine in succession the electrical effects that we have observed in different chemical actions by the aid of the multiplier; *viz.*—1. At the moment of the combination of acids with metals and alkalis. 2. In the dissolutions. 3. In the contact of metallic oxides with the alkalis which combine with them. 4. In the precipitates. As to double decompositions, it has been impossible for me to recognise the slightest trace of electricity at the moment of their formation.

Electrical Effects produced at the moment of the Combination of the Metals and Alkalis with the Acids.

We have seen above that Sir H. Davy observed electrical effects on the contact of acids and alkalis, only when these bodies had been perfectly dried. M. Œrsted asserts that he has perceived them at the instant when the acid combines with the metal.

The following is the means which I employ to shew the electrical effects in these species of actions. I make use of a galvanometer, whose wire is of platinum. (See p. 124 of this volume.) At one of the extremities of this wire I placed a little platinum spoon destined to receive the acid, which is selected of such a nature as not to act on the platinum. To the other end of the wire is adapted a piece of the same metal, between the branches of which (as pincers) the body is placed, which is to act on the acid. In case the platinum could exert an electro-motive action on this body, there is placed between them a bit of moistened paper. Let us begin by shewing what electrical effects result at different temperatures from the contact of a liquid with the platinum. At the ordinary temperature, whatever be the liquid, provided it is not nitro-muriatic acid, the electrical current is null, but when the temperature is raised, phenomena occur which we shall endeavour to explain. Let us put into the spoon distilled water, and let us raise the temperature to ebullition, there will be no current in consequence; if the water of the Seine be used, the current will be extremely feeble, and it will increase in intensity by the addition of a little nitric acid, or alkali. Now, since we know that boiling nitric acid has no more action on platinum than cold nitric acid, it is hence probable that the current is owing to the difference of temperature of the two ends of the wire. It has been already shewn in a former memoir that two pieces of the same metal, in a sufficiently unequal state of temperature, pass, on their mutual contact, into two different electrical states. This change of temperature must therefore be avoided, which is done by using small

fragments of the bodies to be acted on, and a large platinum spoon.

Let us now fix in the platinum forceps a little bit of caustic, soda, or potash, slightly moistened with water. At the moment when the alkali touches the acid, an energetic electrical current will take place, which will proceed from the acid to the alkali following the curcuit. Thus at the instant of contact of these two bodies the acid becomes enveloped with an atmosphere of positive electricity, and the alkali with one of negative. The electrical current is so strong that it may be observed without a galvanometer. It is sufficient for this purpose to present the conjunctive wire to a needle suspended at the filament of a silk-worm. In order to observe the electrical currents which result from the action of an acid on a metal, the same process is employed; care only is taken to prevent the metal touching the platinum directly, by interposing a small slip of paper. The experiment is made in the same way, and the result is the same, whatever be the acid and the base. M. Becquerel next shews that during the solution of a body in water, or alcohol, no electricity is produced. But the smallest acid or alkaline particles are sufficient to modify the results.

He then details some experiments on the solution in caustic potash of metallic oxides, such as oxide of zinc, and of lead newly precipitated. In these, electrical phenomena were exhibited. Whenever the oxide (generally contained in the thin cæcum of an animal,) touches the alkaline solution, the needle deviates from its magnetic direction, and the current goes from the oxide to the alkali, passing along the wire. Hence in these kinds of combinations, the oxides comport themselves like acids, and the alkalis are always surrounded with an atmosphere of negative electricity, as in their actions on the acids.

In slow precipitations, as when an infusion of nut galls acts on sulphate of iron, a current is developed which goes from the infusion to the sulphate. Let us put a solution of sulphate of magnesia in contact with the caustic potash contained in the membranous bag. The needle will deviate slightly from its direction, and the current will be from the sulphate to the alkali. In making nitrate of barytes act on sulphuric acid, the current goes from the acid to the nitrate. When two perfectly neutral salts were employed, as sulphate of soda, and nitrate of barytes, he has not been able to discern the least appearance of a current.

In a subjoined notice, M. Becquerel describes the following experiment. Take a plate of platinum, and placing it horizontally, fix by cement, two glass tubes vertically upon it. Liquids poured into these tubes will communicate through the medium of the platinum plate. Let us pour in any liquids whatever; if they are susceptible of exerting chemical actions on the two ends of the wire of the galvanometer which are immersed, there will be na-

turally established an electrical current, since the plate of platinum permits the electricity to circulate from one liquid to the other. Suppose one of these liquids to be concentrated, and the other dilute nitric acid. On plunging into each tube an end of the copper wire of the galvanometer, the experiment will shew that the electrical current goes from the stronger acid to the other. Let us now substitute, in the place of one of these acids, ammonia, which dissolves the oxide of copper. At the instant of immersion of the two wires the current will go from the acid to the alkali, and will continue to move in the same direction, even when the acid shall be diluted with water.—*Ann. de Chem. et de Phys.* xxiii. p. 244.

HEAT. *On the Property which some Metals possess of facilitating the Combination of Elastic Fluids. By* MM. Dulong *and* Thenard.

After exhibiting to the Academy of Sciences Dobereiner's interesting experiment described in our last number, this gentleman proceeded to detail some modifications of it which they had devised. On immersing some spongy platinum into a mixture of two parts of hydrogen and one of oxygen, explosion takes place. If much azote be present, the water is slowly and actually formed. The sponge of platinum, when strongly calcined, loses the property of becoming incandescent; but in this state it produces slowly, and without any very sensible elevation of temperature, the combination of the gases. Platinum reduced into a very fine powder by a well known chemical process, has no action whatever at the ordinary temperature. The same is the result with wires or laminæ. It might thence be supposed that the porosity of the metal was an essential condition of the phenomenon, but the following facts destroy this conjecture. Platinum was reduced into leaves, as thin as the malleability of this metal allows. In this state the platinum acts at the ordinary temperature, on the mixture of oxygen and hydrogen, with the greater rapidity the thiner its leaf is. They procured some which caused detonation after some instants. But what renders this action still more extraordinary is the physical condition indispensable for its development. A very thin leaf of platinum, rolled round a cylinder of glass, or suspended freely in an explosive mixture, produced no sensible effect at the end of several days. The same leaf crushed together like the wadding of a musket, acts instantaneously, making the mixture explode. Rolled leaves and wires at temperatures of from 200° to 500° cent. act slowly, but without explosion.

Thin leaves of gold and silver act only at elevated temperatures; but always below that of boiling mercury. Silver is less efficacious than gold. In accordance with Sir H. Davy's results with palladium and platinum in the safety-lamp these gentlemen found,

that these two metals, when of the same thickness, acted equally well.

The oxide of carbon and oxygen combine, and nitrous gas is decomposed by hydrogen at the ordinary temperature, when they are in contact of the sponge of platinum. Olefiant gas mixed with a proper quantity of oxygen, is completely transformed into water and carbonic acid by the sponge of platinum, but only at a temperature above 300° cent.

M. Thenard long ago shewed that iron, copper, gold, silver, and platinum, had the property of decomposing ammonia at a certain temperature, without absolving any of the principles of this alkali; and that this property seemed to be inexhaustible. Iron possesses it in a higher degree than copper, and copper more than silver; gold and platinum under equal surfaces. Ten grammes of iron wire are sufficient to decompose, within a few hundred parts, a current of ammoniacal gas pretty rapid, and kept up for eight or ten hours, without the temperature exceeding the point at which ammonia completely resists decomposition. A tripple quantity of platinum wire, of the same size, does not produce a nearly similar effect, even at a higher temperature.

Palladium, in a spongy mass, inflames a stream of hydrogen, as well as platinum. Iridium under this form becomes very hot, with the production of water; nickel and cobalt, in mass, determine at about 300° cent. the union of hydrogen and oxygen; lastly, the sponge of platinum forms, in the cold, water and ammonia, with nitrous gas and hydrogen, and acts also on a mixture of hydrogen and protoxide of azote. M. Gay-Lussac's hydrogen lamp answers well for the experiment of ascension, as the hydrogen would issue in a very small stream. By holding a very light bit of platinum sponge, about three quarters of an inch, before the orifice, the effluent gas is instantly kindled. This is more convenient than the trophorus plate.—*Ann. de Chim. et de Phys.* xxiii. 440.

On the Preparation of Oxide of Uranium. By MM. Lecanu *and* Serbat.

The authors of this process, after having fused the pulverized mineral (pech-blende,) with one half of its weight of nitre, washed the mass which results from the operation, treated the residiuum with nitric acid, evaporated the solution to dryness, and re-dissolved in water acidulated with the same acid, add to the solution an excess of carbonate of ammonia, which, while it is sufficient to re-dissolve the whole oxide of uranium, has no action on the carbonates of lead and lime. M. Laugier, in commenting on the above process, recommends the use of one part and a half of nitre, instead of half a part. The solution containing the nitrate of ammonia, and the carbonate of uranium is to be evaporated to dryness

and calcined, in order to get the pure oxide. M. Laugier advises in preference to wash away with hot water the nitrate of ammonia, and to calcine the remaining carbonate of uranium which has in the filter, a fine lemon yellow colour.—*Journal de Pharmacie*, March, 1823.

On the Oxides of Nickel. By M. J. P. Lassaigne.

The metal was purified by Laugier's process. The protoxide is obtained from solutions in acids, it is of an ash-grey colour, gives green solutions with acids, from which caustic alkalis precipitate it of apple-green colour. Its constituents are

Nickel	100
Oxygen	20

Whence the atomic weight of nickel appears to be 5. The deutoxide is of a brilliant black colour, having some analogy with the peroxide of manganese. At a red heat it gives up a portion of its oxygen, and passes to the state of protoxide. It is prepared by treating the hydrated protoxide with chlorine. M. Lassaigne's experiments on its composition, give

Nickel	100
Oxygen	39·44,

approaching sufficiently near to 40.

The sulphuret artificially made is of a brilliant yellow colour, like iron pyrites, and is very brittle. It consists of

Nickel	100
Sulphur	40

or one atom of each.

He describes a chloride and bichloride, and an iodide, whose constitution may be inferred from the above numbers.

On the Capacity of Saturation of Delphia. By M. Feneulle, *of Cambray.*

Neutral Sulphate—Acid	. . .	3·031	5·0
Delphia	. .	96·969	16·0

Subsulphate—not distinctly characterized. It seems to have a double dose of base. The muriate of delphia is amorphous like the preceding. It is formed of

Muriatic Acid . . .	100	2·136
Delphia	4675	100·000

There is also a submuriate. It consists of

Acid	1·194
Delphia . . .	100·000

Facts subservient to the History of the Succinic and Benzoic Acids. By MM. Lecanu *and* Serbat.

Subjected to the action of heat these acids comport themselves in nearly the same way. They melt, then are volatilized, leaving always a slight carbonaceous residuum. The difference of solubility of these acids in water, as well as in the essential oil of turpentine, establishes a remarkable difference between them. While, in fact, at the temperature of 16° cent., water dissolves scarcely an appreciable quantity of benzoic acid, and, at 100°, only one-twelfth part of its own weight; 100 parts of water, at 16°, dissolve 20 parts. and at 100°, about 46 parts of succinic acid. On the other hand, at the temperature of 16° cent., a gramme of benzoic acid requires for solution only 249 parts of essential oil of turpentine, and at 100° much less than its weight. Hence the liquor, on cooling, concretes into a mass. Succinic acid, even above 100°, dissolves in it very sparingly, although the essence thereby acquires the property of reddening litmus pretty strongly. Hitherto the property of separating iron from manganese, forming with the first an insoluble salt, and with the second a soluble one, seemed to belong only to the benzoic and succinic acids. It is, however, met with in the camphoric and pyrolartaric acids. The last even, would appear even to be capable of separating these metals more completely than succinic acid does. Perhaps it would be advantageous, in regard to economy, to substitute it for this acid.

Succinic acid is not altered by being distilled into nitric acid, diluted with its own weight of water. It is, therefore, not convertible, like some of the other vegetable acids, into the oxalic. Nitric acid becomes thus the most convenient agent for purifying the succinic. The action of nitric acid on the benzoic has not been well investigated, even by these gentlemen; but it is not transformed into the oxalic acid. Succinic acid affords with potash a very deliquescent salt; with soda, a salt unchangeable in the air, or rather somewhat efflorescent, and crystallizing in plates like nitrate of silver; with ammonia, a slightly deliquescent salt, very soluble in water, and crystallizing in long prisms with four faces, transparent and colourless. It occurs frequently in plates; with barytes, a salt hardly soluble, which is obtained in the form of a white powder, by evaporating its solution. They were prevented by an accident from examining the salt that they had obtained with benzoic acid.

These two acids precipitate copper, tin, silver; these precipitates, insoluble in water, are re-dissolved with facility by acetate of potash, and nitrate of soda, without the nitrate of potash, the sulphate and muriate of soda appearing to possess the same property.—*Journal de Pharmacie, for February,* 1823.

Memoir on the Milk of the Cow Tree (Palo de Vaca). By J. B. Boussingault *and* Mariano de Rivero.

Among the astonishing vegetable productions that are met with at every step in the equinoctial regions, a tree is found which yields in abundance a milky juice comparable in its properties to the milk of animals, and which is employed for the same purposes, as M. de Humboldt witnessed at the farm of Barbula (Cordillere littorale de Venezuela), where he drank some of the milky juice. The tree grows in considerable numbers on the mountains which command Periquito, situated to the north-west of Maracay, a village to the west of the Caraccas. The vegetable milk possesses the same physical properties as that of the cow, with the single difference, that it is a little viscid. It has the same taste. In its chemical properties, it differs sensibly from animal milk.

It mixes with water in all proportions, and when thus diluted, it does not coagulate by ebullition. The acids do not convert it into clots, as happens to cow's milk. Ammonia, instead of causing a precipitate, renders it more liquid. This character indicates the absence of caoutchouc. Alcohol occasions a feeble coagulation, or rather renders the juice more easy of filtration. The recent juice slightly reddens litmus. Its boiling temperature is the same as that of water. Exposed to heat, it exhibits at first the same phenomena as cow's milk. A pellicle is formed at its surface, which prevents the disengagement of aqueous vapours. On removing the successive pellicles, and evaporating it at a gentle heat, an extract is obtained resembling frangipane; when the action of heat is longer continued, oily drops are formed, which increase according as the water is carried off, and finally afford an oily liquid, in which a fibrous matter floats which becomes dry and horny, as the temperature of the oil is raised. Then is diffused the best characteristic odour of meat frying in grease. By the action of heat, therefore, the vegetable milk is separable into two parts, the one fusible and of a fat nature, the other fibrous and of an animal nature. If the evaporation of the vegetable milk is not pushed too far, and if the fusible matter be not raised to ebullition, it may be obtained without alteration. It then possesses the following properties:—

It is of a white slightly yellowish colour, translucid, solid, and resists the impression of the finger. It begins to melt at 40° centig., and when the fusion is completed, the thermometer indicates 60°. Alcohol of 40° (sp. gr. 0·817) dissolves it totally by ebullition, and it precipitates on cooling. It saponifies with caustic potash, and with ammonia forms a soapy emulsion. Nitric acid heated on it, dissolves and converts it into oxalic acid. It resembles refined bees' wax, and serves for making candles. The fibrous substance is procured by decanting the melted waxy

matter, washing off the last portions of it with an essential oil, squeezing the residuum, and boiling it a long time in water, to volatilize the oil, the odour of which cannot, however, be thereby completely discharged. Thus obtained, the fibrous matter is brown, having been somewhat altered by the temperature of the melted wax. It is tasteless. Placed on a hot iron, it twists itself and swells up, melts and is carbonized, diffusing the smell of broiled meat. Alcohol does not dissolve it; and hence by treating the extract of the vegetable milk repeatedly with hot alcohol, the fibrous matter is obtained white and flexible. In this state, it dissolves readily in diluted muriatic acid. It possesses the same properties, therefore, as animal fibrine. Fibrine had already been found in the milky juice of the Carica papaya, by Vauquelin. Besides these two main constituents, the vegetable milk contains a little sugar, a magnesian salt (not an acetate), and water. It contains neither caseum nor caoutchouc. By incineration, some silica, lime, phosphate of lime, and magnesia were obtained. The wax forms about one-half the weight of the milk.—*Ann. de Chim. et de Phys.* xxiii. 219.

On the Hot Mineral Waters of the Cordilleras of Venezuela. By the same.

The springs of Onoto issue copiously from gneiss. Their temperature is 44°·5 centig. Their height above the level of the sea, is 702 metres. From the bottom of each reservoir, bubbles of azote rise from time to time in great abundance. The springs of Mariano have a temperature of 44° c., but in particular spots it is from 56° to 64°. They contain a very little sulphuretted hydrogen. They also rise from gneiss, and evolve azote. Silica is the predominating ingredient in solution. Their height above the sea is 476 metres.—*Ann. de Chim. et de Phys.* xxiii. 272.

Physiology.—*On some recent Discoveries relative to the Nervous System. By* M. Magendie.

M. Magendie offers some proofs and illustrations of Mr. Charles Bell's beautiful investigations, on the distinction between the nerves subservient to sensation and motion. An individual had lost the use of his two arms for several years, but he had retained a lively sensibility in these parts. He died, and on examining his body, the posterior roots of the brachial nerves (as they issue from the spine) were perfectly sound, while the anterior roots were evidently altered, had lost their medullary substance, and were reduced to their membranous sheath. The nerves give sensibility or mobility to our organs, only because they are connected with the spinal marrow; whenever they are insulated by a wound, or

any other cause, the part to which they go becomes motionless and insensible. It was, therefore, of consequence to know if the spinal marrow was not itself divided into two halves, the one destended to motion, the other to feeling. M. Magendie has discovered that the spinal marrow is formed, as it were, of two cords juxta-posited, one of which is endowed with an exquisite sensibility, while the other is, so to speak, a stranger to the property, and appears to be reserved for motion. Since it is shewn by the fine experiments of Legallois, that all the other organs, without exception, derive from the spinal marrow their sensibility and mobility, we are led to the remarkable conclusion, that we must cease to seek for any one point in the whole body where the sensibility and mobility are compounded together. Hence it seemed very probable that, in persons who lose the power of moving, while they retain their sensibility, and that reciprocally in those who lose sensibility retaining mobility, there is a disease in the one case of the motive cord of the spinal marrow, and in the other of the sensitive. A lunatic of the hospital of Charenton, had lost, for seven years, the faculty of motion in the whole body, although he retained its sensibility. He died last month. M. Royer Collard, physician to the establishment, made the spinal marrow be examined with the greatest care, and found, in fact, a very marked alteration in the whole motive portion of the spinal marrow, while the portion where sensibility resides was perfectly sound. The centre of the spinal marrow is devoid of sensibility; on touching it, no movements are excited in the body. It is on the surface of this organ, that its properties are developed under the double relation of movement and feeling. Those who think that the electric fluid circulates habitually in our nervous system, may derive from this fact a new argument in favour of their opinion; for electricity diffuses itself, as is known, on the surface of the bodies which it pervades. It is unnecessary to remark, that the facts above related, should have a great influence in the treatment of different palsies. When the cerebral hemispheres of any animal are put out of condition for acting, the animal runs straight forward, with singular rapidity, as if it were pursued. We might say, that an irresistible force presses and precipitates it. If, on the other hand, the action of the cerebellum be stopped, the movements take an entirely opposite direction. The animal draws back; and it is a remarkable phenomenon to see a bird, for example, whose cerebellum has been slightly touched, for whole days make no attempt to walk, swim, or fly, unless it be backwards. It would seem, therefore, to result from these experiments, that an animal in the ordinary state of health, is placed between two forces, which make an equilibrium, of which one would push it in advance, while the other would push it backwards. Volition would have the power of disposing at its option of these two forces.

A disease of the horse, little known, was proper to verify the precision of these results. Veterinary surgeons call this disease *immobility;* and, in fact, when it is wished to make the animal seized with it, fall back, whatever effort be employed, and whatever means be taken, it stands motionless. The forward movements are, on the contrary, easy, and seem sometimes to occur even without the participation of the will. If the inference which I have drawn be exact, the disease ought to consist in a physical alteration of the cerebrum, or in some obstruction of the action of this organ. I caused to be examined, last month, two horses attacked with immobility, and the conjecture has been completely verified. In both the cerebrum was visibly altered; the cerebellum, on the contrary, was unaffected. It appears, then, to be demonstrated, that the two opposite motive forces of the cerebrum and cerebellum exist in animals, and that, in certain cases, they may be withdrawn from the influence of the will. M. Magendie relates a case of a man somewhat similarly affected, who was cured by some grains of sulphate of quinina.—*Ann. de Chim. et de Phys.* xxiii. 429.

Prussiate of Iron as a Cure of Intermittents.

Doctor Zollickoffer, of Baltimore, has employed this substance, and his success has been as remarkable as with cinchona.—*Journ. de Pharm. July,* 1823.

Injection of a Solution of Opium into the Veins of an Hysterical Patient. By Charles W. Coindet.

This experiment was made at Edinburgh, and the result was such as to deter any young physiologist from repeating it in hysteria. The patient was seized with violent spasms, constituting a case of idiopathic tetanus. They commenced very regularly by attacks of emprosthotonos, the head frequently striking the knees with force. Opisthotonos succeeded; the body took the form of a bow, and rested only on the heels and occiput. All the muscles of the body participated in this state of painful tension, which, one time, lasted twenty-seven minutes. The respiration was performed with difficulty, the pulsations of the heart became feeble and irregular, and the young girl (fourteen years of age), was threatened with suffocation. This horrible agony was succeeded by some convulsions of pleurosthotónos, which terminated the paroxysm. Dr. Coindet dissolved a scruple of common opium in an ounce of distilled water, heated to the temperature of 80° cent. At half-past seven in the evening he began the injection, assisted by his friends MM. Hercy and Lucius O'Brien. He made an opening in the right basilical vein, with an ordinary lancet as for blood let-

ting. He removed the bandage from the arm; he then introduced the pipe of a syringe, and threw a drachm and a half of the solution into the vein, taking care to exclude every portion of air, though the experiments of Nysten had shewn that a few air bubbles would occasion no mischief. The breathing was immediately affected, becoming more regular, less rapid, and less convulsive. The pulse and other symptoms remained as before. The successive injections were repeated at intervals of five minutes. At the second, the breathing became quite natural; the pulse rose to 100, and was fuller. The skin became slightly coloured, and was soon covered with a faint perspiration. The spasms lost their violence; she heaved one or two sighs, like a person coming out of a profound sleep. After the fourth injection she recovered her hearing, but not her sense of sight. At the fifth, she began to see, and articulated some phrases distinctly. The operation was not followed with any disagreeble symptoms. On the following day, the girl described her sensations with much clearness. At every injection, it appeared as if a torrent of fire had been poured into her veins, which rising up her arm, and following the course of the vessels, which she pointed out very exactly, passed under the clavical of the same side, and concentrated its operation for some instants on the chest, whence it proceeded to the head and along the back, from which it diffused itself through the whole system, and produced lively prickings and an intense heat in the skin. She spoke of her sensations as having been very painful. After six weeks of convalescence, she relapsed into a similar state of disease to that for which the injections were used. She finally recovered from the convulsive affections by sea bathing; but was afterwards seized with swelling of the mesenteric glands.

Dr. Coindet says, we must not expect from opium injections any thing more than the temporary cessation of the spasms, whereby the stomach may be brought back to its natural functions, which interval must be taken advantage of, for administering the suitable remedies, by the customary passages.—*Bibliotheque Universelle, May* 1823.

Economics.—M. Viney one of the editors of the *Journal de Pharmacie*, has given, in the number for February last, the following recipe for making a fetid and bitter solution, capable of destroying all kinds of insects:—

Take of wood mushrooms, or large brown fetid boletuses	6 pounds
Black soap	2 ——
Grated nux vomica .	2 ounces
Water	200 pounds

The mushrooms bruised and beginning to putrefy, are to be put into the water holding the soap in solution. The mixture is to be left to putrefy in a cask for some days, care being taken to agi-

tate the liquid from time to time. When it has become very fetid, the decoction of the nux vomica in water is to be poured in. This liquor is employed to sprinkle the objects from which insects are to be repelled, whether in gardens or elsewhere, taking care not to use it on gildings or polished metals, which it would blacken. The insect cannot stand this fetid poison.

Art. XIV.—MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *Remarks on Iron Wire Suspension Bridges.*—The following remarks on this subject are from a memoire by M. Dufour, the Engineer of the Geneva bridge, briefly mentioned at page 369 of the last volume of this Journal: they are naturally connected with the account of that bridge.

Speaking of the comparative strength of iron in wires and in bars, (see p. 367 last vol.,) M. Dufour says, "The immense advantage of employing iron in wire rather than in bars, is thus rendered evident: it is more manageable, its strength is double, the strength may be better proportioned by putting the number of wires necessary to the resistance required, and a certainty is obtained of the state of the interior parts of the suspending lines, which nothing can give when large bars are used."

"It appears at first that the minimum of the force of the wire should be calculated upon, and not the mean; but as each bundle contains many wires, although there may be some of a smaller strength, there will be others that will surpass in strength, and thus the mean should be used in estimating the strength of the whole, although in employing a single wire the minimum only ought to be taken."

With regard to the Geneva bridge, M. Dufour says that after a period of four months in which the bridge had been in full use, it had not suffered the slightest alteration in its primitive form. The path has retained the degree of curvature given it at first, and no sensible lengthening of the wires has occurred. The bridge, however, has been well tried, curiosity has taken great numbers of persons on to it at once, and all the large stones required in the latter part of the work, were taken over it on carriages without the slightest damage. The elasticity of the bridge is also what it was at first, a man walking with a moderate step does not at all disturb the steadiness of the path; on walking quickly there are slight vibrations produced, but no oscillations, and the vibrations are such as never to be communicated from the one bridge to the other, or in any way to affect the masonry.

The expense of the bridges was as follows :—

Masonry of the abutments, *&c.*	4100 francs.
———— lodges, stations, *&c.* . . .	3800
Forged iron, *&c.*, for the gates	2800
Iron wire and workmen	1940
Wood-work required, workmen, *&c.* .	2250
Lead, copper, tin, varnish, *&c.* . . .	800
Terraces for the parapets, foundation, *&c.*	160
Various expenses	500
	16,350

Bib. Univ. xxiv. 297.

2. *Test for the action of Frost on Building Materials, by* M. P. Brard.—MM. Lepeyre and Vicat knowing that I had been long occupied in the study of mineralogy as applicable to the arts, engaged me in an investigation of the means best adapted to distinguish such stones, as, being otherwise fit for building materials, gave way to the action of frost. I found it impossible in this respect to ascertain any thing from their mineralogical characters, and was obliged to follow another course. During the winter of 1819, I carefully examined with a lens the chalky limestone of the neighbourhood of Perigueux, and the sandstone of the coal basin of la Vezère, both equally liable to this action, I soon found that each scale of the limestone, and each grain of the sandstone was raised by the re-union of small needles of ice, which when they melted suffered the particles to fall and collect about the stone, and that where particles had fallen off in this way, a fresh succession was raised in the same manner, and ultimately separated from the mass.

I was struck by the resemblance of the ice in silky crystals to the saline efflorescences which appear between the plates of certain shists and on the surface of old walls. I remembered the effect of common salt on bad pottery, and on the saline rocks of the Tyrol, and conceived the idea of substituting the action of a saline solution to that of common water. After various experiments, I gave the preference to sulphate of soda, its effects being the most constant and most comformable to the action of frost.

The experiment that it may lead to satisfactory results should be conducted as follows. Suppose an excavation newly made into limestone or other rocks, and it be desired to ascertain the liability of the rock to disintegration by the action of frost.

1st. A cube of two inches in the side is to be cut from each part to be tried; the various cubes numbered with thick China ink, and their original sites also marked.

2nd. About four pints of common cold water is to be saturated with sulphate of soda, so that a few grains of the salt shall remain undissolved.

3rd. This solution is to heated to ebullition, and then all the cubes to be entirely immersed in it. When the boiling has recommenced it is to be continued for half an hour.

4th. The cubes are to be withdrawn from the solution and placed each one in a saucer, numbered as the cube is; a small quantity of the solution is to be poured on to each cube, and the whole left until covered with white efflorescences perfectly analogous in appearance to the rime or hoar frost, which causes the disintegration of the stones. These efflorescences will appear in about twenty-four hours if the air is dry or hot, but in a humid atmosphere are sometimes five or six days.

5th. When the efflorescences appear on the angles and sides of the cubes, they are to be dissolved again by means of a few drops of water, or better still with a little of the solution in which the cubes were boiled. If well managed the efflorescences will soon re-appear, and when well formed, are again to be removed in a similar way, and this is to be repeated for three or four days together*; after which each cube may be washed with abundance of common water, but without removing it from the saucer.

6th. The specimens to be tried having been washed on all their faces, the detached matter is to be examined, and a judgment formed from it, of the relative qualities of each kind of stone submitted to the proof: for the greater the number of the detached particles collected in the saucer, the more liable is the stone to be attacked by frost; the smaller the number the more capable is it of resisting the action.

As yet, all the results of this test have accorded perfectly with the effect of time and frost. Such stones as have been found to disintegrate by frost have given way to the salt, such as time has sanctioned have resisted the new agent, so that the mechanical effects of the two are perfectly analogous. Crystallization takes place with both, augmentation of volume, efforts on the surfaces of the small cavities containing the water or solution, and if the aggregation be not sufficiently powerful to resist the action, disruption, and a gradual decay of the rocks either in their natural sites, or if they have been applied to use in their new situations. The action of the sulphate of soda being quite mechanical, is exerted indifferently on all kinds of rocks deficient in aggregation, on limestones, sandstones, large grained granite, granites of too micaceous a structure, shists, lavas, *&c.* It may be employed as a proof or test also even upon slates, bricks, tufas, mortars, and cements, as is proved by a table of various results of this kind.

* If the proof be continued for a longer period, good building stones may be rejected, for the prolonged action of the salt is more powerful than that of ice.

The above is freely translated from a paper inserted by M. Brard, in the *Bib. Univ.* xxiv. 224.

3. *On the Strength of Cast Iron and other Metals.*—It was our intention to have noticed a new edition of Mr. Tredgold's valuable essay on the above subject, in a former part of our Journal, but this has been prevented by the pressure of other matter. We must, therefore, rest satisfied with laying before our readers the contents of the eleven sections into which the work is divided, reserving to a future occasion a more explicit account of its contents.

The *First Section* consists of introductory remarks on the use and the qualites of cast iron; and of cautions to be observed in employing it. This section is followed by three extensive tables, which will often save the practical man a considerable share of trouble in calculation.

The *Second Section* explains the arrangement and use of the tables, which precede it; and in this edition, the number of popular examples is much increased.

It is a common and a well understood fact, that an uniform beam is not equally strained in every part, and therefore may be reduced in size, so as to lessen both the strain and the expense of material.

The *Third Section* points out the value of cast iron, in this particular, and the forms to be adopted for different cases.

The *Fourth Section* contains a popular explanation of the strongest forms for the sections of beams; the construction of open beams; and the best forms for shafts. A due consideration of these two sections will enable the young mechanic to guard against some common errors in attempting to apply these things to practice. They are much augmented, and a new principle of constructing bridges is explained in the fourth section.

The *Fifth Section* is wholly devoted to experiments on cast iron; it will be found to contain, in addition to the author's experiments, almost all of the experiments that have been described by preceding writers.

To this section a great many new experiments have also been added, to show the relative strength of iron of different qualities; and also seven new experiments on torsion, made by Messrs. Bramah. The section concludes with the result of the author's observations on the relation between the appearance of the fracture and the strength of cast iron as determined by experiment.

The *Sixth Section* contains experiments on malleable iron and other metals, and is entirely new. The effect of hammering and the decrease of force by heat, are experimentally examined; and

the cause of English iron being inferior to Swedish, for particular purposes, is pointed out.

In the *Seventh Section* we are shown how to obtain some of the most useful practical rules from the first principles that are furnished by experience.

The *Eighth Section* treats of the stiffness to resist lateral strains, with its application to some interesting practical cases.

The *Ninth Section* is on the strength and stiffness to resist torsion or twisting, with its application to machinery.

The *Tenth Section* treats of the strength of columns, pillars, and ties, with some new examples. It may be useful to remark, that the most refined methods of analysis have been applied to the same subjects by Euler, Lagrange, and other continental mathematicians, without arriving at results more accurate, more simple, or more convenient in practice.

In the *Eleventh Section* the author considers the resistance of beams to impulsive force. In this section will be found many important rules, with examples of their application to the moving parts of engines, bridges, *&c.*, wherein the advantage gained by employing beams of the figures of equal resistance is shown.

The Eleventh Section is followed by an extensive *Table of the Properties of Materials, and other Data, often used in Calculations*, arranged alphabetically, and in this Edition much enlarged. By means of this table the various rules for the strength of cast iron, contained in this work, may be applied to several other kinds of materials.

A Note, added at the end of the table, on the chemical action of some bodies on cast iron, will be read with interest by those who employ cast iron where it is exposed to the action of sea water.

4. *On the Capillary Action of Fissures, &c.*—M. Dobereiner has remarked a singular effect produced apparently by fissures. Having filled a large glass flask with hydrogen, and left it standing over water, it was observed some days after, that the water had risen in it above one-third of its capacity. The only cause for this effect that could be assigned was, the existence of a very minute fissure in the glass. Filled a second time and left over water, the fluid had risen in it above an inch and a half in twelve hours, and in twenty-four had risen two inches and three quarters, during which time the barometer and thermometer had not sensibly altered. In other experiments, vessels of other forms were used, and the water uniformly rose in those having fissures.

When one of these vessels filled with hydrogen was covered by a bell glass, or when the vessels were filled with atmospheric air, oxygen, or azote, instead of hydrogen, no change took place.

M. Dobereiner considers the effect as due probably to capillary

action. He suggests that all gases may be considered as consisting of solid atoms of various sizes, enveloped by atmospheres of heat also very different, and that hydrogen though it has the largest atmosphere of heat, has the smallest atom, and is thus permitted to escape by fissures, which retain the other gases. "Probably," he says, "fissures may be formed which will permit azote to pass, but not oxygen, and others again which will let the oxygen out, but not carbonic acid gas."

Another experiment which seems related to this subject is as follows:—A thermometer-tube had been drawn out very fine in the lamp, and it being desired to have it filled with alcohol, the point was immersed in that fluid, and the bulb heated until no more bubbles of air escaped; the tube was then cooled, but no alcohol entered. When again heated abundance of bubbles of air passed out through the alcohol, though when recooled no alcohol would enter. Upon examining the tube with a lens, nothing was seen which could prevent the entrance of the alcohol; on withdrawing the tube from the alcohol, the external air entered with a hissing noise. M. Dobereiner conceives that the diameter of the tube was so small that the alcohol could not enter, but only the air which it contained.—*Ann. de Chim.* xxiv. 332.

5. *Sound produced by opening a Subterraneous Gallery.*—In the road made by Napoleon communicating between Savoy and France, and which passes by Chamberry and les Echelles, there is, as is well known, about two miles from the latter place, a gallery cut in the solid rock, twenty-seven feet high and broad, and nine hundred and sixty feet in length. Mr. Bakewell states in his travels, that this gallery having been commenced at both ends, when the excavations from each end nearly met, and the thin partition of rock between them was first broken through by the stroke of the pick, a deep and loud explosion followed resembling thunder. The cause of this explosion Mr. Bakewell thinks is easily explained. The air on the eastern side of the mountain being sheltered both on the south and west from the sun's rays, must be frequently many degrees colder than that on the western side. The mountain rises full one thousand feet above the passage, and at least fifteen hundred feet above the bottom of the valley, forming a partition between the hot air of the valley, and the cool air of the ravines on the eastern side, and a sudden opening being made for the dense air to rush into a rarer medium, must necessarily produce a loud report, just as a bladder does upon bursting in the rare air of a receiver. The sound of the explosion being greatly increased by reverberation through the long archway on each side.—*Bakewell's Travels.*

This explanation of the origin of the sound seems insufficient to us, inasmuch as it would require a much greater difference of ba-

rometric pressure on the opposite sides of the previously existing partition of rock than probably existed.

6. *Nautical Eye-tube.*—A trial has been made on board the Clio among the Orkneys, and in the Moray Frith by Mr. Adams, of the performance of his eye-tube to the telescope of a sextant for taking altitudes when the horizon is invisible. In making the observations the horizon was always screened from the instrument, and under these circumstances after rejecting a few observations the mean difference of 199 altitudes of the sun, moon, and stars, taken by the eye-tube, from those taken at the same time in the ordinary way by the officers of the Clio, and corrected for dip, amounted to only 1′ 10″. The altitudes taken by the eye-tube are not affected by any dip or depression of the horizon. Considerable care and practice is required in the use of the instrument, but that attained, the latitude, the time at the ship, and consequently the longitude may all be determined by it when the horizon is invisible. By means of it also either the large or the pocket sextant may be employed on shore as a substitute for the theodolite, upon making the necessary allowance for the parallax of the instrument in the name of index, error, which on becoming sensible, must vary inversely with the distances of the reflected terrestrial objects.—*N. M. Mag.* xii. 16.

7. *Leghorn Straw Plait.*—The Dublin Society having offered premiums for the best imitations of Leghorn plait, awarded three prizes to successful candidates. Not less than twenty-four specimens were exhibited from widely remote parts of Ireland. The finest specimen was made from *avena flavescens*, or yellow grass, by Miss Collins of Plattin, near Drogheda. The second was made of *cynosurus crystatus*, or crested dog's tail, by Miss Grimley of Kiltinon, near Newton Mount Kennedy. The third of *agrestis vulgaris*, or common bent grass, by Miss Campbell of Londonderry.

II. Chemical Science.

1. *On Fulminating Silver and Mercury.*—The following results are collected from a memoire on these substances, by Dr. Just Liebeg, which has appeared in the *Annales de Chim.* xxiv. 294.

The fulminating silver was obtained by dissolving about 60 grains of fine silver in half an ounce of nitric acid, spec. grav. 1.52, adding two ounces of alcohol of spec. grav. .85, and heating slowly in a flask until ebullition commenced; in a short time, white crystalline flocculi appeared, the vessel was removed from the source of heat, and left to cool. The ebullition continued some time, and

the deposit augmented. The vessel should not be cooled hastily in this process, as great loss of the compound is occasioned.

Thus prepared, the fulminating silver appears in white silky acicular crystals, possessing the well known detonating properties, perfectly soluble in 36 parts of boiling water, and re-crystallizing as the solution cools. It has a metallic taste, stains the skin, if exposed to air becomes first red, then black, and to test papers appears as a neutral salt.

Fulminating mercury was prepared according to Howard's process: 100 grains of mercury being dissolved in half an ounce of concentrated nitric acid, and two ounces of alcohol added. Heat is then to be applied, as in the former case. At first a little nitrate of mercury is deposited, but is soon redissolved, and then on a sudden the liquor becomes grey from the reduction of part of the oxide of mercury, and the liberation of a dense vapour, occasioned by the volatilization of a portion of mercury with the ether that rises. After some time the liquid becomes yellow, and dendritical crystals appear, which augment on cooling until nearly a quarter of an inch in length. They are greyish-white, harsh to the touch, and heavy, but when purified by being dissolved and crystallized two or three times, appeared as perfectly white brilliant silky crystals, having a mild metallic taste, and detonating violently by a blow. They are pure fulminating mercury.

On adding lime-water to fulminating silver the latter dissolved, leaving a little black oxide of silver; when a few drops of nitric acid were added to the clear solution a white precipitate fell, which detonated like the original compound. It now dissolved without any residue in lime-water, and was precipitated again by acid, as before, without any indication of decomposition.

Substituting potash for lime-water, and boiling, exactly the same effects took place. The fulminating silver combined also in the same manner with magnesia, baryta, strontia, soda, and ammonia, and with all of them presented the same phenomena, except that ammonia did not cause the separation of oxide of silver. The quantity of oxide separated by the alkaline bases from 100 of fulminating silver was 31.25.

Thus it appears that fulminating silver perfectly resembles a compound salt; its acid combines with the alkalies, &c., and its base, the oxide of silver, separates; and in confirmation of this view of its nature it was found that compounds of the acid, and all other bases, might be obtained perfectly definite and crystallized, and possessing strong detonating properties.

A quantity of fulminating silver was decomposed by lime, the liquid filtered, concentrated, and carefully precipitated by nitric acid, excess of the latter being avoided. The new acid, when well washed, appeared as a white powder, very soluble in boiling water, reddening litmus paper, and crystallizing on cooling.

Researches were then made into the nature of this acid: the term *fulminate*, has been applied to the salts containing it. Muriate of potash added to fulminate of potash produced no precipitate of chloride of silver; but muriatic acid decomposed the salt, and chloride of silver, muriate of ammonia, hydrocyanic acid and carbonic acid, resulted. Fulminate of potash does not precipitate persulphate of iron, nor does the addition of muriatic acid form a prussiate of iron. Metallic copper precipitates all the silver from fulminate of potash, and a plate of zinc indicates the copper present; but excess of potash does not separate the copper, nor does the fluid become blue by adding ammonia, though when the solution is decomposed by muriatic acid, the copper is easily found by those tests. Chromates, prussiates, and carbonates, do not precipitate the silver from alkaline fulminates; these properties point out a strong analogy between this acid and the metalliferous cyanic acids.

The fulminating acid boiled with oxide of silver, gave fulminating silver; boiled with oxide of mercury, it produced a compound in small brilliant plates.

Conceiving from analogy that the acid of fulminating mercury differed from that of fulminating silver in the substitution of the former metal for the latter, experiments were made to ascertain this point; crystals of fulminating mercury boiled with potash, deposited oxide of mercury, and the fluid, when precipitated by nitric acid, gave a white precipitate, which, when dry, detonated strongly by percussion; with baryta, strontia, and lime, similar compounds to those formed by fulminating silver, were produced. The separation of the acid from fulminating mercury does not always succeed. In only two operations out of eight was the acid obtained in yellow detonating crystals.

A quantity of fulminating silver was put with metallic mercury into water and boiled; after some time the liquid became turbid, it was filtered, and furnished crystals exactly the same as those produced by the acid of fulminating silver and the oxide of mercury. Boiling another portion for a much longer time, the precipitate deepened in colour, and when no more was formed, the whole was filtered and crystallized. The crystals were very fine, and pure fulminating mercury; and an amalgam of mercury and silver remained. The reverse operation was performed of preparing fulminating silver from fulminating mercury; the latter was boiled with silver which had been precipitated from the nitrate by copper, and to which a quantity of platina filings had been added; by the galvanic action of the two metals the mercury was precipitated, and the silver dissolved. The experiment requires rapid manipulation and simple decantation, otherwise the crystals will always contain mercury.

Fulminating silver was boiled with copper; the silver precipitated,

and the liquid, which was found to contain copper, after some time deposited a bluish-green powder, which behaved like a true combination of oxide of copper with the acid of the fulminating silver, containing copper in place of silver. The compound detonated more feebly than that of silver, and was difficultly soluble in boiling water; on evaporating the mother liquor, a large quantity of fulminating copper was obtained. Zinc gave similar results, but more rapidly. Iron also produced a crystallized fulminating compound.

When fulminating mercury was acted on by the metals, similar phenomena were produced; and fulminating copper and fulminating mercury were thus obtained.

When fulminating silver was boiled with magnesia, the liquid was found to contain but very little of the acid, but a reddish precipitate had formed, which, though it contained the greater part of the fulminating mercury, merely decrepitated feebly when thrown upon a hot coal. Half an ounce of it heated in a retort, decomposed quietly, yielding a portion of carbonate of ammonia and water, and carbonic acid gas, no other gas being liberated. In order, therefore, to obtain a knowledge of the constituents of fulminating silver, 100 parts were well mixed with 400 parts of calcined magnesia, and heated in a luted retort, the products being carefully received and estimated. They were,

	With fulminating silver	With fulminating mercury
Carbonic acid	35·5	25·8
Ammonia	13·7	10·0
Water	7·2	5·2
Silver	41·0	Mercury 56·9
Loss	2·6	2·1
	100·	100·

These being the mean of four experiments on each compound. The only substance which varied was the carbonic acid, and the proportions of the other substances remained constant. These gave as the ultimate elements,

	Fulminating silver	Fulminating mercury
Oxygen	32·22	23·39
Hydrogen	3·22	2·34
Nitrogen	11·28	8·23
Carbon	9·68	7·04
Silver	41·00	Mercury 56·90

The following are some of the compounds of the acid of fulminating silver with bases.—*Magnesia* combines in two proportions with the acid: one is a rose-coloured powder, not soluble or detonating, but merely decrepitating by heat; the other is in beautiful white filamentous crystals, resembling capillary silver, and

strongly detonating.—*Baryta* combines with the acid apparently in two proportions; the first crystallizes in dull white grains, which detonate powerfully, and are difficultly soluble in water.—*Strontia* resembles baryta in its compounds.—*Zinc* forms a salt in small yellow granular crystals, very soluble, and very heavy.—*Potash* produces a salt which crystallizes in long white brilliant plates, having a disagreeable metallic taste, not affecting test-paper, dissolving completely in eight parts of boiling water, and detonating powerfully when heated or struck. It contains 85·08 of acid, and 14·92 of base.—*Soda* has always produced a salt in small rounded plates, brown and brilliant; they are lighter and more soluble than the preceding, but otherwise resemble it. They contain 88·66 of acid, and 11·34 of base.—*Ammonia* with fulminating silver leaves no residuum. Berthollet's compound being formed at the same time with the other. On cooling, a large quantity of granular crystals are obtained, which are difficultly soluble, and have a strong metallic taste. They detonate even in the liquid when touched by a glass rod, but fortunately if excess of alkali be present the detonation does not extend to the neighbouring portions.

2. *On the unequal Dilatation of a Crystal in different directions, by heat.*—On measuring the mutual inclinations of the planes of a crystal of carbonate of lime at different temperatures, M. Mitscherlich observed that they varied sensibly with the temperature, the variation sometimes amounting to 8′.5 from 32° to 212. Fahr. When the temperature rose, the obtuse diedral angles diminished, or in other words the short axis of the rhomboid expanded more than the other diagonals, so that its form approached to that of the cube. M. Mitscherlich concluded, therefore, that the double refraction of the crystal would at the same time diminish; a result confirmed by an experiment which he afterwards made with M. Fresnel in the manner adopted by that philosopher in 1817, to render more sensible the changes in the tints of plates of sulphate of lime. M. Fresnel had then observed, that elevation of temperature sensibly diminished the double refraction of sulphate of lime; and according to the recent experiments of the two philosophers the same effect is produced, though in a much less degree, on rock crystal. This experiment, however, requires repetition.

It appears, therefore, that generally an uniform elevation of temperature in a crystal diminishes its double refraction. M. Mitscherlich thinks that heat ought always to separate the molucules of a crystal farthest apart in that direction in which they are most contiguous.—*Ann. de Chim.* xxv. 109.

3. *Difference of crystalline Forms of the same Substance.*—M. Mitscherlich, who first observed the remarkable fact that a body may affect two different crystalline forms, has, in a memoir on this

subject, quoted sulphur as an instance. Natural crystals of sulphur are furnished by some calcareous strata, and by volcanoes. Artificial crystals may be obtained either by evaporating a solution of it in carburet of sulphur, or by fusion of the sulphur and slow cooling. On fusing native sulphur, it gives the same crystals as common sulphur. The primitive form of the crystals of sulphur, either natural, or obtained as above by evaporation, is an octoëdron, with a rhombic base; but the primitive form of the crystals obtained by fusion, is an oblique prism, with a rhombic base.—*Ann. de Chimie*, xxiv. 264.

4. *Supposed Effect of Magnetism on Crystallization.*—The following is an experiment first made by Professor Maschmann, of Christiana, and confirmed by Professor Hanstein, of the same city; we should not have noticed it but for these names. A glass tube is to be bent into a syphon, and placed with the curve downwards, and in the bend is to be placed a small portion of mercury, not sufficient to close the connexion between the two legs; a solution of nitrate of silver is then to be introduced until it rises in both limbs of the tube. The precipitation of the mercury in the form of an arbor Diana will then take place, slowly only when the syphon is placed in a plane perpendicular to the magnetic meridian; but if it be placed in a plane coinciding with the magnetic meridian, the action is rapid, and the crystallization particularly beautiful, taking place principally in that branch of the syphon towards the north. If the syphon be placed in a plane perpendicular to the magnetic meridian, and a strong magnet be brought near it, the precipitation will recommence in a short time, and be most copious in the branch of the syphon nearest to the south pole of the magnet.

4. *On Thermo-magnetism.*—The following account of results on the magnetism of a single piece of metal developed by heat, is abstracted from a paper by Dr. J. d'Yelin, or rather from an account of that paper in the *Bibliotheque Universelle.* The results, if confirmed by further experience, are very highly important to the theory of magnetism.

In repeating the experiment of Seebeck, M. Yelin made use of platina, gold, silver, iron, copper, brass, zinc, tin, lead, antimony, bismuth, and arsenic. The result of his observations was that "the effect of Seebeck's circuit should not be considered as a determinate function of power possessed by the heterogeneous metals of developing electricity by contact, and of their various conducting powers as to heat; and that therefore, conclusions cannot be drawn from these properties," as is proved by the following facts:

1. Silver and zinc give by contact an electricity stronger than silver and antimony; but a circuit formed of the two latter metals

has much greater power than one composed of the former, which is very feeble. The case is the same with the two sets, copper and zinc, and copper and bismuth.

2. Brass, copper, and lead, according to Bockmann, have a conducting power as to heat of 344, 346, and 850; nevertheless, a circuit of brass and copper is sensibly stronger in its action than a circuit of lead and copper.

3. Finally, silver in contact with antimony is electrized negatively, in contact with zinc it is still more powerfully so; but other circumstances being equal, a circuit of antimony and silver has seven times the power over a magnetic needle that a circle of zinc and silver has. Antimony is positive when opposed to platina, gold, or silver, and negative when opposed to copper, tin, lead, or zinc; but whichever of those metals be formed into a circuit with antimony, the same effect is obtained, the same pole of the needle always being urged to the same side. Bismuth and antimony are both positive when in contact with platina, gold, and silver, but all other things being equal, as the dimensions of the metal, the soldering, the temperature and arrangement, a circuit formed of bismuth and one of the last named metals, turns the pole of the needle 14°, 51°, or 45° to the east, whilst if antimony be substituted for the bismuth, the pole is thrown 18°, 25°, or 30° to the west.

Being induced to consider the rupture of the equilibrium of temperature as the principal cause of the electro-magnetic action of Seebeck's circle, M. d'Yelin endeavoured to obtain similar effects with a single piece of metal, and having obtained very decided effects, he has given to this class of phenomena the name of thermo-magnetism. That very feeble magnetic action might be observed, very delicate needles were used; they were of great tenuity and suspended by a single spider's thread.

If a band of any single metal be formed into a circuit of any figure, by riveting one of its ends near the other, and the projecting end be heated by a flame, whilst the circuit is plunged in cold water, this band will become electro-magnetic, and its properties may be easily ascertained. The experiment was made with zinc, bismuth, brass, tin, lead, and copper, and M. d' Yelin infers that "all metallic bodies acquire electro-magnetic properties when their various parts are unequally heated, and that the action is stronger as the difference of temperature is greater."

This fundamental experiment being established, the following are the principal results obtained by the author:—

I. The metals, in reference to their thermo-magnetic properties, may be ranged as follows, commencing with those which possess them in the highest degree, bismuth, antimony, zinc, silver, platina, copper, brass, gold, tin, lead.

II. A metal acts differently on the needle according as the hot or

the cold part of it be placed under the needle. The following experiments were made with cast bars six or seven inches long, one inch in thickness, and formed either as cylinders, or as prisms with three, four, or six sides; solid and hollow balls were also employed:—
1. If one extremity of a bar of bismuth be heated, the bar be placed in the direction of the magnetic needle, with its cold end to the north, and the hot end be brought under the needle, the point of the needle will turn towards the east. 2. If the direction of the bar being preserved, it be moved towards the south until its cold end is under the needle, the needle will turn towards the west. 3. The inverse effects are obtained when the hot end of the bar is towards the north. 4. When the bar is heated in the middle, and the ends preserved cold, the same effects are obtained for each half of the bar. 5. The magnetic effects are sensible when one part of the bar is heated merely by the hand and the other cooled by snow.

III. The magnetic action of metals unequally heated depends on the form given them in casting, and in this it differs from the action of Œrsted's connecting wire. 1. If an equilateral triangular prism of bismuth be used as in the former experiments (1), and its faces be turned upwards successively, one of its faces will make the needle deviate to the east, the next face (that towards the east) brought into the place of the first, will make the needle deviate to the west; the third face has so uncertain an effect that it may be considered as null. 2. If a square or four-sided prism of bismuth, antimony, or zinc, be used in a similar manner, it will be found that two contiguous faces when turned upwards will make the needle move eastward, whilst the other two faces will move it westward, so that the prism may be considered as composed of two triangular prisms of which the un-magnetic faces are in contact. 3. With a regular hexagonal prism three of the faces move the needle eastward, and three move it westward. 4. Cylinders present peculiar effects; a cylinder of bismuth had been thrown with its mould into cold water immediately after being cast, another was suffered to cool slowly; when these cylinders were used in place of the prisms, the ends which were uppermost in the moulds being placed under the needle, one part of the curved surface urged the needle to the east, and the other part to the west; these parts were equal in the first cylinder, but unequal in the second. When the other extremities of the cylinders were placed under the needle, then the curved surface of the first cylinder presented four nearly equal portions which successively turned the needle to the east and west: the second bar presented six similar portions.

The differences remarked between the extremities of the cylinder, and also between the cylinders themselves, when cooled slowly or rapidly, induces M. d'Yelin to conclude there is some relation between the crystallization of metals and their magnetic properties.—*Bibliotheque Universelle*, xxiv. 253.

5. *Electromagnetic Multipliers.*—Dr. Kaerntz has lately been occupied in proving experimentally the amount of the advantage obtained in electromagnetic multipliers, by each additional circumvolution of the wire. His motor was a zinc plate about eight inches long and four inches wide, the copper opposing both sides was consequently double that size. The fluid conductor was a solution of muriate of ammonia in spring water, with the addition of one hundredth of sulphuric acid. The connecting wire was copper harpsichord wire, covered with silk thread, and the same length was used in every experiment. By connecting the plates with the wire before immersion, by immersing slowly and by other expedients, any important variation in the intensity or quantity of action was avoided.

In this way it was found that the quantity of power of the instrument over the needle, was exactly in proportion to the number of convolutions, six convolutions giving six times the power of one convolution; and by experiments, when the forces of the instrument and of the earth's magnetism were arranged in different ways, this result was confirmed. Such an instrument is therefore more correctly called a multiplier than a condenser.—*Phil. Mag.* lxii. 441.

6. *Plate Electrical Machines.*—A variation in the construction of plate electrical machines has been devised and practised by M. Metzger of Siblingen in Schaffhouse, which would seem to be a real improvement. Considering that the effect desired in using the machine was first highly to excite the glass, and then to collect the electricity from it, M. Metzger concluded that the distance between the rubber and the points of the conductor in machines of the common construction was injurious in its effect, not only by causing the dispersion in part of the electricity excited, but by uselessly wasting the exciting surface. Plates were therefore mounted in a very compact and perfect manner, with three pairs of rubbers placed at equal distances from each other; the conductor also had three arms furnished with points a little in advance of each pair of rubbers, to collect the electricity in the usual manner. The rubbers were not attached to a surrounding frame, but to brass arms, which proceeding from a socket through which the axis passes, diverged at equal distances from each other towards the periphery of the plate. The machine has a very compact and neat appearance, and its various smaller parts are contrived with much judgment.

In some comparative experiments made with a plate twenty-two inches in diameter, the superiority of three pair of cushions over two pair was very manifest. In the following table the first column expresses the length in inches of the rubbers; the second the length of the spark when two pair of rubbers were used, and the

third the length of the spark when three pair of rubbers were on the machine.

6 inches . . .	12 inches . . .	18 inches.
7 „ . . .	14 „ . . .	21 „
8 „ . . .	16 „ . . .	24 „
9 „ . . .	18 „ . . .	27 „
10 „ . . .	20 „ . . .	30 „

Bib. Univ. xxiv. 187.

7. *Improvement of the Leyden Jar.*—M. Metzger has also varied the construction of Leyden jars, so as to augment their capacity without increasing their apparent volume. For this purpose having two jars of proper dimensions, he simply places one within the other, so that they shall apply pretty correctly, and thus have a capacity of charge nearly proportional to the whole surface of coating, without increasing the volume of the whole beyond that of the larger jar. Jars made slightly conical would answer well for this purpose.—*Bib. Univ.* 191.

8. *Electricity on Separation of Parts.*—In the water-proof cloths manufactured by M. Mackintosh of Glasgow, where two pieces are cemented together by caoutchouc dissolved in coal tar oil, the adhesion is such that when the two are torn asunder in the dark, there is a bright flash of electric light, similar to that produced by separating plates of mica, by breaking Rupert's drops, or by breaking barley-sugar, or sugar candy. Upon trying this experiment with different substances, it was found that flashes of light were distinctly produced, by tearing quickly a piece of cotton cloth.—*Edin. Jour.* x. 185.

9. *Electric Light.*—Having a metallic wire covered with silk, form it into a close flat spiral, taking care that the revolutions touch each other. Their number may be arbitrary, more than twenty-four have not been used. The properties of this spiral when it forms part of the voltaic circuit are well known, but pass through it a charge of common electricity, such as may be taken by two square feet of coated surface, moderately charged, and a vivid light, something resembling that of an artificial fire-work, will occur, originating from the centre of the spires. It may be seen very distinctly without darkening the chamber where the experiment is made.

M. Leopold de Nobili, who describes this experiment, considers the phenomenon as perfectly new. If the wire be folded backwards and forwards, so as to form a rectangular surface, then the electric discharge only produces a faint light at each corner, and this he considers as the light produced by the escape of the electricity into

the atmosphere; but the light from the spiral is said to be so vivid and distinct, that once seen its dissimilarity from the former must be instantly evident. He has, therefore, called it electromagnetic light, because of its relation to the magnetic state of the spiral, thinks that it might be made continuous if a sufficiently powerful voltaic battery were used, and has little doubt but that the aurora borealis is such a light elicited by the magnetic state of the earth. —*Bib. Univ.* xxv. 38.

10. *Connexion of Phosphorescence with Electricity.*—The sulphate of quina was shewn by M. Callaud d'Annecy some time since to become highly phosphorescent when rubbed at a temperature of 212°. MM. Dumas and Pelletier have ascertained that it becomes highly negatively electrical when rubbed on woollen cloth, and hence were led to the verification of a suspicion they had long entertained that phosphorescence was an electrical phenomenon. About two or three ounces of sulphate of quina were introduced into a glass flask, and heated for half an hour in a water bath at 212° F., it then by friction gave out a sufficiently intense light. The flask was closed by a cork, through which passed a wire pointed at the inner extremity, and terminated by a ball at the external end; on approaching this ball, two or three times to the knob of a voltaic electrometer furnished with its condenser, having taken care to shake the flask before each contact, the leaves became so electrical as to diverge as much as the instrument would admit of, the electricity being constantly positive.

The sulphate of cinchona, which is phosphorescent like the sulphate of quina, though less so, also became electrical in the same manner. Its electricity, though of the same kind, was not so strong as that of the preparation of quina.—*Ann. de Chim.* xxiv. 171.

11. *Phosphorescence of Acetate of Lime.*—Dissolve any quantity of acetate of lime in water, and place it on a sand-heat in a Wedgewood ware dish, evaporate to dryness without disturbing it. When quite dry, let the bulb of a thermometer be rested on the bottom of the dish, and when the temperature has attained 250° F., the lime will be found to adhere very firmly. If light be now excluded, and the acetate be strongly rubbed with a stiff spatula, it will become highly luminous. Mr. N. Mills.—*Ann. Phil. N. S.* vii. 235.

12. *Preparation of Sulphurous Acid Gas.*—M. Berthier has shewn that this gas may be obtained very pure and abundantly, by heating a mixture of twelve or fourteen parts of sublimed sulphur, and a hundred parts of peroxide of manganese in a glass

retort. The residue in the retort is not a sulphuret of manganese, but a protoxide of manganese mixed with a little sulphate of manganese, and sometimes a little sulphur.—*Ann. de Chim.* xxiv. 275.

13. *Preparation of Sulphuretted Hydrogen.*—The experimental researches of M. P. Berthier, into the production and composition of certain metallic sulphurets, have been referred to at length, vol. xv. p. 148. Since then another paper has appeared by the same chemist, and relating to the same subject, from which we extract the following matter. In the preparation of sulphuretted hydrogen, it is usual to act on sulphuret of iron by diluted sulphuric acid, or sulphuret of antimony by strong muriatic acid; but, for various reasons, M. Berthier recommends the following compounds of sulphur as better. Powdered common iron pyrites is to be mixed with half its weight of dry carbonate of soda, and heated red-hot in a crucible; a fluid sulphuret of iron and sodium is obtained, which may be poured out on a stone to cool, and is then a homogeneous deep yellow mass, possessing a lamellar fracture. It absorbs much water, forming with it a black paste, which when acted on by sulphuric or muriatic acid instantly yields abundance of sulphuretted hydrogen; leaving a black sulphuret of iron, which by the application of acid and heat, will yield a second portion of the gas.

Peroxide of manganese mixed with sulphur or charcoal, and heated to bright redness, becomes a protoxide, which treated with sulphuric acid forms a sulphate. This sulphate powdered, mixed with one-sixth of powdered charcoal, and heated to whiteness in a closed crucible, yields a pulverulent sulphuret of manganese, which when acted on by a mixture of one part sulphuric acid, and one part water, gives abundance of sulphuretted hydrogen, and becomes sulphate of manganese again: one hundred parts of the sulphuret yields 38¼ parts of sulphuretted hydrogen.

Of all the sulphurets that of calcium appears to be the most proper for this purpose. It produces abundance of the gas, a hundred parts producing 46.8 of sulphuretted hydrogen; the residue on the action of muriatic acid is entirely soluble, and therefore admits of perfect and free action without the application of heat; and it may be obtained in the greatest abundance. Sulphate of lime is to be reduced to an impalpable powder, and then mixed with powdered charcoal in the proportion of 0.15 of the latter, if the sulphate be a hydrate, but if anhydrous 0.20 of charcoal will be necessary. The mixture is to be put into crucibles, and heated to whiteness for an hour or two in a wind furnace. The sulphuret does not act on the crucible, and is obtained in a pulverulent state. If the sulphuret be required in great quantity it may be prepared by mixing the sulphate of lime and charcoal with a sufficient quan-

tity of plaster of Paris, moistening and moulding the whole into bricks, which may be burnt like bricks of clay.—*Ann. de Chim.* xxiv. 271.

14. *Preparation of Saturated Hydro-sulphuret of Potash or Soda.*—The following is M. Berthier's process: mix ten sulphate of potash, ten sulphate of baryta, and five powdered charcoal, or eight dry sulphate of soda, ten sulphate of baryta and five powdered charcoal, and heat them to whiteness in a crucible. Double sulphurets are obtained, which are greyish, half-fused, and easily separated from the crucible; they contain each an atom of the composing sulphurets. Pulverize and introduce them gradually into a flask three-fourths filled with warm water, close it and frequently agitate it; when saturated, the water will contain an atom of the sub-hydrosulphuret of alkali and an atom of sub-hydrosulphuret of baryta. Diluted sulphuric acid is then carefully added to the solution in the flask, by small portions at a time, agitating each time and preserving the flask well closed. In this way the baryta is precipitated, and its sulphuretted hydrogen goes to the alkali; when all the earth has fallen, the fluid is left to become clear, is decanted and tested by solution of salts of lime or magnesia. If a precipitate occurs, a fresh portion of hydrosulphuret of baryta must be added to the liquor and precipitated by sulphuric acid. With a little care, a neutral hydrosulphuret of the alkali is obtained, which contains neither baryta or sulphuric acid, but of the two it is better to have the acid in excess.—*Ann. de Chim.* xxiv. 279.

15. *Preparation of Kermes Mineral.*—According to M. Fabroni, a much finer kermes mineral is obtained by using tartar in place of the alkali employed in the usual process. Three or four parts of tartar should be mixed with one part of powdered sulphuret of antimony, and heated red in a crucible until the cessation of fumes indicates that the tartar is all decomposed; the mass is then to be dissolved in hot water, filtered and left to cool, when abundance of fine kermes will be deposited, of a very deep colour. The abundance of kermes thus obtained does not at all interfere with the quantity and beauty of the golden sulphuret, afterwards obtained by the addition of acid to the mother liquor.—*Ann. de Chim.* xxv. 7.

16. *Action of Sulphur on Iron.*—Col. A. Evans has remarked, that although sulphur has so strong an action on heated wrought iron as immediately to form holes in it, yet it does not at all affect grey cast iron. A plate of wrought iron, 63 of an inch in thickness, heated to whiteness, and held against a roll of sulphur $\frac{6}{10}$ of an inch in diameter, was in fourteen seconds pierced through with perfectly cylindrical hole. Another bar about two inches in

thickness was pierced by the same means in fifteen seconds. Good steel was pierced even more rapidly than the iron, but a piece of grey cast iron, well scaled and heated till nearly in fusion, was not at all affected by the application of sulphur to its surface, not even a mark being left. A crucible was made of this cast iron; and some iron and sulphur put into it; on applying heat the iron and sulphur soon fused together, but the cast iron underwent no change.—*Ann. de Chim.* xxv. 107.

17. *Economical Preparation of pure Oxide of Nickel*, by M. Berthier.—Speiss, or impure nickel, is to be reduced to fine powder and roasted till it gives off no further vapours of arsenic, the heat being at first moderate to prevent fusion, and then increased. Metallic iron in the state of filings or nails is to be added in a quantity which ought previously to be determined, and the whole dissolved in boiling nitro-muriatic acid, so much nitric acid being used that no protoxide of iron remain in the solution; evaporate to dryness and re-dissolve in water, when a large quantity of arseniate of iron will be left. Add to the solutions successive portions of carbonate of soda until a greenish precipitate appears, at which time all the arsenic and iron will be separated, and part of the copper; the rest of the copper may be separated by sulphuretted hydrogen, and the clear solution thus obtained, when boiled with sub-carbonate of soda, yields the carbonate of nickel.

Thus obtained, the carbonate of nickel contains a little cobalt; to separate the latter, the precipitate as obtained above by boiling with sub-carbonate of soda, is to be well washed and diffused whilst moist in water, and a current of chlorine passed into it until in excess: the excess of chlorine is to be allowed to dissipate and the solution filtered; it now contains not the smallest trace of cobalt, that remaining as a hydrated peroxide, with a certain portion of nickel in the same state. If in the mixed carbonate of nickel and cobalt, the latter is in excess; the residue, after the action of the chlorine, is pure hydrate of cobalt, and the solution contains the nickel with a small quantity of cobalt.—*Ann. de Chim.* xxv. 95.

18. *White Copper.*—According to M. Keferstein, a metallic composition resembling silver has been employed under the name of white copper, for a long time at Suhl, in ornamenting fire-arms. M. Brandes, by analysis, found it to be an alloy of copper and nickel. MM. Keferstein and Muller have recently sought out the origin of this substance, and have ascertained that it is found in the scoria of some ancient copper-works, formerly attached to mines now abandoned. The white copper, which had formerly been rejected as useless, is now obtained by fusion, for the purpose above stated.—*Ann. de Chim.* xxiv. 234.

19. *Prussian Blue.*—Mr. Badnall, of Leck, has taken out a patent for improvements in dyeing with Prussian blue. The improvement consists in preparing the Prussian blue, by mixing it in fine powder with strong muriatic acid, and stirring it until the whole becomes a smooth homogeneous mass of a semi-gelatinous consistence. We notice it here merely to remark on the circumstance that an agent in which Prussian blue is insoluble, should be found useful in enabling it to combine with silk, cotton, wool, &c. The pure ferroprussiate of iron is soluble in water, but the addition of a small portion of muriatic acid immediately precipitates it; wash away the acid by pure water, and the pigment becomes soluble again; re-acidify, and it re-precipitates.

20. *Crystallization of the Sub-carbonate of Potash.*—Il Dot. M. Fabroni describes the following process for the crystallization of this salt. Make a solution of pearlash in water, and evaporate it until of specific gravity 1.57. Allow it to cool, when all extraneous salts will be deposited; separate the fluid and again concentrate it until of specific grav. above 1.6. The fluid will now be of a light green colour, and strong alkaline odour; place it in deep vessels, as glass jars for instance, and the sub-carbonate will soon crystallize in long rhomboidal white laminæ, situated vertically and parallel to each other; one extremity will touch the bottom of the vessel, and the other be attached to a saline crust on the surface of the liquid. When cold, the mother liquor will be found of specific grav. 1.6, but if further concentrated and again cooled, more crystals will be obtained; and this may be continued until the whole has been crystallized.—*Gior. di Fisica*, vi. 451.

21. *Composition of Ancient Ruby Glass.*—Mr. Cooper, on analyzing a portion of this glass, sent to him by Mr. C. Muss, found it to contain silex, oxides of copper, iron, and silver, and lime. He considers the oxides of copper and silver as the colouring matter, but from the coloured portion being a film not more than $\frac{1}{200}$ of an inch in thickness, upon the surface of the glass, it was impossible to ascertain their proportions. Iron existed abundantly in the uncoloured portion of the glass. Mr. Cooper thinks the alkali used as a flux for the siliceous matter is soda.—*Ann. Phil.* N. S. vii. 106.

22. *Detection of Arsenic in cases of Poisoning.*—Mr. Phillips, in a very excellent practical paper on the methods of employing the various tests proposed for the detecting the presence of arsenic, has very much facilitated their use in certain cases, by pointing out that where the arsenic is mingled with a complicated mixture of animal and other substances, as when its presence is to be ascertained in fluids from the stomach, animal charcoal may be very advantageously employed as a preparatory agent. Some coloured

liquor arsenicalis on being boiled for a few minutes with ivory black was rendered so colourless that any of the tests for arsenic could be readily applied. The experiment was repeated, substituting for the colouring liquor, port wine, gravy-soup, and a strong infusion of onions, and in all these cases a solution was obtained sufficiently colourless for the application of the most delicate tests. This agent is not liable to any mistake from the presence of phosphates, for water or wine boiled in it alone separated nothing except, in one or two cases, a small portion of a muriate; to avoid the interference of this substance, the ivory black may be washed with boiling distilled water until the washings do not affect nitrate of silver; but good ivory black does not require this treatment.

With regard also to the application of sulphate of copper as a test of arsenic, Mr. Phillips recommends a precaution which has not heretofore been thought of. Sulphate of copper yields a green precipitate when added to potash, white arsenic being present; but if the sulphate contains any peroxide of iron, it may yield a green precipitate with the alkali, the arsenic not being present. Mr. P. has remarked that the arsenic may be added after as well as before the precipitate is formed, for the blue precipitate occasioned by the potash in pure sulphate of copper becomes green when the white arsenic is added. Add solution of potash first, therefore, to the sulphate of copper, and obtain the fine blue precipitate; to a part of this add the suspected solution, and if arsenious acid be present it will convert the blue precipitate into a green one.—*Ann. Phil.* N. S. vii. 30.

In reference to the reduction of arsenic to the metallic state, as a test of its presence, Dr. Trail thinks the general opinion of the large quantity required is unfounded, and easily succeeds in obtaining this evidence from $\frac{1}{10}$ of a grain of white arsenic. The tube is to be 2½ inches long, 0. 4 inch. wide, and closed at one end. The substance thought to be arsenic should be mixed with thrice its weight of black flux, or sub-carb. soda mixed with charcoal powder, introduced into the tube, and a little charcoal powder put over it; the upper part of the tube must be cleaned, and the mouth closed by a piece of paper. The flame of a spirit-lamp will in about two minutes produce a shining metallic crust on the upper side of the tube; when cold shake out the loose materials, scrape off the metallic crust, which will afford sufficient for six different portions, each of which when projected on a dull red-hot poker will give a white smoke and alliaceous odour. A clean knife held in the smoke will always condense a portion of white powder.—*Ann. Phil.* vii. 132.

23. *On the Detection of Acetate Morphia in cases of Poisoning, by M. J. L. Lassaigne.*—The following are the processes recommended. If the acetate of morphia be suspected in a liquid, it is

to be evaporated by a moderate heat, and the residue digested in alcohol, which will dissolve the acetate as well as ozmazome and some salts. The alcoholic solution evaporated, and the residue dissolved in water, will cause the separation of a portion of fatty matter. The last solution is to be evaporated spontaneously, and if it contains acetate of morphia, that substance will crystallize in diverging needles of a yellow colour, and known by, 1, their bitter taste; 2, their decomposition by ammonia; 3, the liberation of acetic acid by strong sulphuric acid; 4, the red colour developed by nitric acid. If the salt be in such small quantity that the ozmazome prevents its crystallization, nitric acid will detect it by the colour produced.

If it is suspected to exist in a solid mixture or substance, it is to be boiled with water for about ten minutes, and then treated as above. If the accompanying substances are alkaline, a small quantity of acetic acid must be added, to form an acetate with the morphia.

By these methods M. Lassaigne has detected the acetate of morphia; 1, in the substances vomited by animals to which it had been given; 2, in the stomach of a cat who died on taking five grains of it; 3, in the liquid from the thorax of a dog which died ten minutes after the injection of fourteen grains of the substance; 4, in the small intestines of a cat which died ten hours after the injection of eighteen grains of the substance into that canal; 5, in the duodenum of a dog which died four hours and a half after the injection of eighteen grains into that part.

It was found also in the blood from the jugular vein of a horse, opposite to that by which thirty grains of the acetate had been injected ten minutes before; but five hours after the injection none could be found, indicating that where the animal could support the poison it was gradually destroyed or expelled. A grain of the salt mixed with six ounces and a half of ox blood, was easily found again after several hours.

Lest the orange colour produced by nitric acid should be due to the presence of an animal substance, M. Lassaigne endeavoured to avoid the presence of any such matter, and found the following process perfect in this respect. A solution of sub-acetate of lead is added to the aqueous solution of the alcoholic extract suspected to contain acetate of morphia, all the colouring and azoted matters are immediately precipitated, and there remains in solution only certain salts with the acetate of morphia, and a slight excess of acetate of lead, which latter may be decomposed by a few bubbles of sulphuretted hydrogen. The solution should then be evaporated in vacuo over sulphuric acid, and if it contains acetate of morphia, that substance will soon crystallize, its base may be separated, and the colour by nitric acid is no longer equivocal.

The conclusions appended to this mémoire, are, 1, that it i possible in many cases of poisoning by acetate of morphia, to discover sensible traces of this vegetable poison; 2, that the substances vomited shortly after taking the poison into the stomach contain ponderable quantities of it; 3, that it is always in those viscera into which it has been introduced, that its remains may be detected; 4, that all attempts as yet made to discover it in the blood of the dead animals have been fruitless.—*Ann. de Chim.* xxv. 102.

24. *Test for Morphium*—M. Dublane a druggist of Paris, states that he finds the tincture of nutgalls a very sensible test of the presence of morphium in fluids, whether it exist free or in combination with acetic or sulphuric acid.—*Ann. de Chim.* xxv. 92.

25. *Process for obtaining Strychnia, by M. Ferrari.*—Boil three pounds of bruised nux vomica for two hours in thirty pints of water acidified by six ounces of muriatic acid, and pass the liquid through a cloth or sieve: the residue should be thrice boiled again for the same time, and in equal quantities of acid and water. To the united cold infusions add lime slowly whilst mixing, until in considerable excess; after two or three days decant the liquid, collect the paste on a filter, dry and powder it. The decanted liquor should be rather more than neutralized by muriatic acid, evaporated until reduced to a few pounds, when cold precipitated by lime, allowed to stand, decanted, and the residue when dry, powdered and added to the former.

Sulphuric acid may be substituted for muriatic acid in the process, but the three pounds of nux vomica will require only three ounces of this acid in twenty pints of water, and the boiling should continue one hour only. Afterwards, the liquid rendered acid is to be concentrated until like syrup, being agitated during the evaporation if any deposit appears. To the cold liquor powdered lime is to be added, and the process goes on as before.

The mixed precipitate of lime and strychnia obtained by either of the above methods is to be heated in a water-bath two or three times, with alcohol of specific grav. 0.832, until all the bitter principle is extracted. The united fluids are to be distilled, and this operation finished; there will remain a yellow turbid bitter alkaline fluid, which is to be decanted off and reserved, and beneath it the strychnia soiled by a yellow colouring matter, which will harden upon cooling. This mixture treated with cold alcohol of specific gravity .915 will leave pure strychnia.—*Gior. de Fisica.*

26. *Volatility of Salts of Strychnia.*—Il Sig. Ferrari has remarked that solutions of salts of strychnia slightly acid when exposed to a heat of 212°, so as to be concentrated, then become volatile and the salt evaporates. This property has been remarked in the sul-

phate, nitrate, muriate, and acetate, and is believed to belong to all the salts. It has been remarked by M. Collaud and others, that the sulphate of quina is also volatile, and M. Ferrari, on repeating the experiments with the muriate and nitrate of quina, found it also to happen with them. The solutions on being heated in a tinned copper vessel, gave out vapours which when breathed, were found to be highly bitter. The salts vary in the extent of this property, and it is also affected by the degree of acidity, and of concentration of the solution.—*Gior. de Fisica*, vi. 460.

27. *Acid Tartaro-Sulphate of Potash.*—Il Sig. M. Fabroni says that sulphuric acid being boiled with thrice its weight of water, and cream of tartar in excess, gives a fluid, which after having been evaporated, cooled, and allowed to deposit undecomposed tartar, sulphate of potash, *&c.*, will not furnish any other deposit, and resembles oil in its appearance. When further evaporated to the consistence of syrup, and again cooled, it solidified in a mass composed of imperfect prismatic crystals, and which when dry, had something of the appearance of camphor. It dissolves rapidly in water, but in alcohol yields its tartaric acid, and acid sulphate of potash is left. On analysis it gave seventy-two tartaric acid, and twenty-eight acid sulphate of potash. M. Fabroni thinks that for many uses this salt may be a cheap and effectual substitute for tartaric acid. He considers it as analogous in its nature to the compound of tartar and boracic acid.—*Gior. de Fisica*, vi. 452.

28. *Pyroligneous Ether, or Pyroxilic Spirit.*—A brief description is given* at p. 436, vol. xiv, of this Journal, of a substance obtained by Mr. P. Taylor, first in 1812, and at various times since then, from the distillation of wood. M. Taylor called it pyroligneous ether. Latterly this substance has been re-examined with great care, by MM. Macaire and Marcet, of Geneva, who have called it pyroxilic spirit, in their paper upon it, published in the *Bibliothèque Universelle*. The following is a brief account of their observations. The fluid is transparent, colourless, of a strong ethereal odour slightly resembling that of ants. Its taste is hot and strong, leaving an impression on the tongue like that of essence of mint: its specific gravity .828. It boils at about 150° F. Its slightly acid properties appear to be due to a little acetic acid. It burns away entirely with a perfectly blue flame. Alcohol dissolves it in all proportions, but water separates it again. With water only, it forms a sort of emulsion, which is of considerable permanence. It does not dissolve in oil of turpentine. It dissolves camphor, but not oil of olives either hot or cold. It also dissolves pure potash.

Heated with its volume of sulphuric acid, it distils over un-

* From the *Phil. Mag.* LX. 315.

changed; heated with thrice its volume of sulphuric acid, it blackens, swells, and liberates a small quantity of inflammable gas, which burns with a pale flame, is not condensed by chlorine, and appears to be a mixture of proto-carburetted hydrogen and hydrogen. When distilled with its volume of nitric acid nitrous vapours arise, and an ethereal liquid distils over, which when distilled from oxide of lead, reddens litmus, has an agreeable odour, burns with a strong greyish flame, dissolves in water and alcohol, communicating a sweet mild taste, and in all its properties quite unlike nitric ether. A current of nitrous gas passed into a portion of the pyroligneous fluid effected no change in it. Muriatic acid produced no effect upon it.

A current of chlorine passed into the fluid, made it of a deep yellow colour, but continuing the current a few minutes, the colour on a sudden disappeared, six parts of the fluid had thus increased to six parts and a half, of a colourless transparent liquid, fuming by ammonia, having a poignant odour, and exciting tears. It burnt with a blue flame, producing abundant fumes of muriatic acid, and an odour resembling horseradish. When distilled from litharge, it passed over less acid, but otherwise unchanged. Its specific gravity was 0.889; it was soluble in water and alcohol, communicating a strong taste of horseradish. It precipitated nitrate of silver, and became more acid by exposure to air and light. This compound as well as that produced by the action of nitric acid, appears to be an ether, having particular properties; and these ethers prove that the pyroligneous fluid is in its relation to acids analogous to alcohol.

MM. Macaire and Marcet then prepared some of the pyroacetic spirit described by Chenevix, and instituted comparative experiments on it, and Taylor's fluid. The pyroacetic spirit is lighter than the pyroligneous fluid, being according to Chenevix of specific gravity 0.786. Its taste and smell are different. It burns with a strong white flame, and is quite soluble in oil of turpentine. Sulphuric acid does not trouble or blacken it, but produces a fine yellow red colour, and the fluid remains transparent until heated. Distilled with muriatic acid, a volatile fluid passes, and a black substance remains; distilled on potash the fluid loses its acid odour, and the residue smells like tar. Chlorine passed into the pyroacetic spirit, rendered it of a slight yellow colour. The fluid resulting had a strong suffocating smell, resembling that of the substance obtained by treating the pyroligneous fluid in the same way, but after a time it separated into two portions, the one thick, oily, heavy, and transparent, the other light, and slightly opalescent. The latter burnt with a light blue flame, being an acid residue. It is soluble in water, communicating a hot taste to it, but not like horseradish. The oily fluid burnt

with a dense green flame, and the production of much muriatic acid, it was soluble in alcohol, but insoluble in water, at the bottom of which it lay in drops.

Finally, MM. Macaire and Marcet proceeded to analyze these fluids, and this they effected by oxide of copper: one hundred parts of the pyroxilic spirit or pyroligneous ether gave

44.53	of carbon, or	6	atoms.
46.61	oxygen,	4	„
9.16	hydrogen,	7	„

One hundred parts of the pyroacetic spirit of Chenevix gave

55.30	of carbon, or	4	atoms.
36.50	oxygen,	2	„
8.20	hydrogen	3	„

Analyzing alcohol of specific gravity .820 at the same time, one hundredparts gave

48.8	of carbon, or	3	atoms.
39.9	oxygen,	2	„
11.3	hydrogen	5	„

The conclusions of the mémoire are, 1st. That there exist at least two simple vegetable fluids distinct from alcohol, but like that liquid, having the property of forming with acids, particular ethereal spirits; 2nd. That these two fluids which may be distinguished by the names pyroacetic spirit and pyroxilic spirit, differ from each other, both in their properties and composition.—*Bib. Univ.* xxiv. 126.

We suspect some mistake in the printing of the figures of the analysis, for there is no accordance in the estimation of the weight of the atoms deduced from them. If the weights expressed be divided by the number of atoms assigned, and the whole be reduced to the atom of hydrogen, as unity it gives the weight of an atom of carbon 5.67 by the first analysis, 5.06 by the second, and 7.2 by the third, and the weight of an atom of oxygen as 8.9 by the first analysis, 6.67 by the second, and 8.82 by the third.—*Ed.*

29. *Cafeine.*—Cafeine is a crystallizable principle discovered in 1821, in coffee, by M. Robiquet, whilst searching in it for quina. MM. Pelletier and Caventour obtained this substance at the same time, but did not complete their researches. M. Robiquet read a mémoire on this subject to the Société de Pharmacie of Paris, which has not been published. It is, however, known to be a new principle, white, crystalline, volatile, and slightly soluble.—*Dict. de Med.*

Its composition is very remarkable, for according to MM. Dumas and Pelletier, it consists of

Carbon . . .	46.51
Nitrogen . .	21.54
Hydrogen . .	4.81
Oxygen . . .	27.14
	100.

The quantity of nitrogen in it surpasses that of most vegetable substances.—*Ann. de Chim.* xxiv. 183.

30. *Conversion of Gallic Acid into Ulmin*—The following statement is by M. Doebereiner. On dissolving a determinate quantity of gallic acid in ammonia, and placing the solution in contact with oxygen, it absorbed sufficient to convert all the hydrogen of the gallic acid into water. In this way the acid became converted into ulmin, which is composed of

1	atom . . .	12 carbon
1	,, . . .	1 hydrogen
2	,, . . .	16 oxygen

and may be represented as a combination of two volumes of gaseous oxide of carbon, and one volume of vapour of water.—*Ann. de Chim.* xxiv. 353.

There is some mistake in the above statement of the composition, but the fact is very curious— *Ed.*

31. *An Account of an Electrical Arrangement produced with different Charcoals, and one conducting Fluid, communicated by Mr. T. Griffiths.*—In the course of some experiments on charcoal, the results of which are given in a late number of the Journal of Science, two specimens were obtained differing remarkably in mechanical texture, and electrical conducting power, but more especially in the former. One of them being soft and porous, absorbing water with great avidity; the other hard and compact, absorbing it with comparative slowness. I was induced from the observation of this fact, to try if it would be possible to form an electrical arrangement of several such pieces made into arcs and plunged into glasses of water; supposing that the absorption of that fluid taking place more rapidly in the one than the other, might at the time develope electricity.

An apparatus was accordingly constructed, consisting of several pieces of the two charcoals united by a wire into the form of an arc, and dipping at their extremities into glasses of pure water. Upon connecting its opposite ends with the tongue, a perceptible taste was experienced similar to that produced by a very feeble

galvanic combination; the limbs of a newly-killed frog underwent evident convulsions when made part of the circuit.

In order to remove any source of fallacy that might have attended the employment of a metallic wire to conneet the charcoals, another apparatus was made in which they were united into arcs by cotton or silk threads, and upon examining it by the tongue and the limbs of a frog, the effects were similar to those before produced in the first form of experiment.

Upon making a tube filled with water part of the circuit, a decomposition was expected, but none took place, although the experiment continued several hours, nor would it revive copper from its solution in sulphuric, or acetic acids, so that its respective poles have not been distinguished.

In all experiments made with this apparatus, the employment of metals was carefully excluded, so that their contact with the charcoal should not give incorrect results.

If when the limbs of a frog are undergoing convulsions, one of the arcs be removed from the circuit, they instantly cease, but return again, upon its being replaced; and it is a curious fact that the effect on the limbs is decidedly most powerful, when the nerve is in contact with the rapidly absorbing surface; if the opposite arrangement be adopted making the muscle in contact with it, the effect is greatly diminished, or altogether ceases.

That the activity of the apparatus is dependent upon the absorption of water, is proved by its cessation in about twenty-four hours, the charcoals becoming saturated with water; but by heating them red hot it is expelled, and upon again arranging them in the manner mentioned, they will be found to regain their former activity.

A solution of common salt, being employed as the fluid, augments the effect of the apparatus, it being a better conductor; but if it is wished to heat the charcoals for another experiment, they should be soaked in water to dissolve the salt, which would otherwise fuse, and fill up the pores. The woods from which the charcoals are obtained, are known by the names of Botany Bay, and King wood. The former should be chosen full of dark streaks, which open when exposed to heat, and give the resulting charcoal a great degree of porosity. In selecting the other wood, no very particular attention is required, it generally producing a charcoal of pretty uniform density.

III. Natural History.

1. *Vegetation at different Heights.*—The following is a table constructed by Mr. Bakewell, of the height at which various trees and shrubs grow in the Vallois and Savoy. The extreme height

implies situations open to the south and west, and sheltered from the north-east wind, the height varying very much according to the aspect in an alpine country. The heights are expressed in English feet above the level of the sea, lat. 45° 30′ to 46° 30′.

Vines	2380
Maize	2772
Oak	3518
Walnut tree	3620
Yew tree	3740
Barley	4180
Cherry tree	4270
Potatoes	4450
Nut tree	4500
Beech tree	4800
Mountain Maple	5100
Silver Birch	5500
Larch	6000
Fir le sapin	6300
Pinus cembra	6600
Rhododendron	7400

The line of trees reaches the height of 6700, the line of shrubs 8500. Some plants on a granitic soil grew at 10,600, above which are a few lichens, but vegetation ceases at 11,000. In the Garden of the Inn, kept in summer at the Schwarrenbach, on the passage of the Gemmi, carrots, spinach, and onions, are cultivated at the height of 6,900 feet.

In the southern part of Savoy, the height at which pines will grow is about 2,600 feet, but near this elevation the crops failed in the cold summer of 1821.—*Bakewell's Travels.*

2. *Irritability of Plants.*—Whilst experimenting on the irritability of certain plants, as the sensitive plant for instance, Dr. Meyer had occasion to observe, that of those substances which acted by being absorbed into the plant, the most volatile were also the most powerful, although not the most destructive. When the extreme leaflets of a branch were moistened with naphtha or essential oil, the influence gradually extended itself to the neighbouring leaflets, and even to the other leaves of branches. Their recovery was in the inverse order of their depression. Another observation by the same author on these plants is, that when affected by a trembling motion the leaflets close, but if the motion be continued for some hours they will again open.—*Bib. Univ.* xxv. 53.

3. *Notice of an undescribed Larva which attacks and devours Snails.*—The account of this larva was read before the Society of Natural History of Geneva, by Count Milzinsky. As far as the

Count could ascertain by reference to books, and by inquiry amongst those best acquainted with insects, it had never been described. The insect was five or six inches long, and two or three broad; its colour yellow; it was furnished with two long bifurcated mandibles; had at the upper part two brown antennæ, each composed of two articulations, and supported on a white membraneous projection; beneath the mandibles were four feelers, two of them in constant motion. The body is divided into twelve rings, the three anterior have each two strong feet, and but little hair; the following eight have each two false feet, and on each side two tufts of hair; the twelfth has two large terminal tufts of hair, which serve as a case to a sort of cartilaginous tail which the animal moves at pleasure and uses as a sort of supplementary foot: it is hollowed at the extremity, and covered with a viscid humour. Between the lines formed by the tufts are two ranges of projecting glandular black points, considered by M. Milzinsky as trachia.

The larva is excessively voracious, attacking and apparently feeding entirely on snails. On meeting with a snail, if the animal be out of its shell, the larva takes a position on the shell and does not attack the snail until it has entirely entered its habitation; the larva then approaches the right side of the snail and forcibly plunges his head into it, helping itself powerfully by the use of the hind foot. The snail gives evidence of suffering, and endeavours to withdraw into and go out of its shell, moving much about, but in a short time it ceases its motion and dies. The means by which the larva produced so quick a death to the snail could not be ascertained, for all passed so much within the shell as to be withdrawn from observation. During the time that the larva remains in the body of the snail, either alive or dead, only the terminal tufts of hair are seen without. The larva will sometimes in this manner attack and destroy three snails in one day.

These insects are generally found in dry ditches or by hedges. If a snail's shell be observed that has recently fallen, and the first spire be broken, one of these animals will almost certainly be found within. They vary in size, and are proportionate to the snails in which they are found. A small larva, on devouring a snail, grows considerably, changes its skin, and then searches for a larger snail. When it has attained its final size it attacks its last snail, rejecting with force, towards the middle of its operation, a semi-liquid decomposing matter; and by the time it has eaten or emptied all the contents of the shell (the shell remaining clean) it has become large, white and shining; it then remains inactive for a variable portion of time, afterwards changes its skin, but in a manner different to the previous changes, and becomes a chrysalis. In this state it remains awhile, and preserves its tufts, but less apparent than in the former state. The chrysalis remains at the bottom of

the shell for two or three months, and then on a sudden becomes white; shortly the spots and colours of the skin appear, and the insect ultimately passes into its perfect state, when it deposits its eggs. All these changes take place within the shell, and it is difficult to ascertain them without disturbing the animal and deranging the results.

Drawings of the animal having been shewn to MM. La Freille and Audouin, they are inclined to believe that the insect in its perfect state is not merely a new genus, but a particular family, which they would place in the order of Thysanours, or in that of parasites.—*Bib. Univ.* xxiv. 137.

4. *Hatching Fish.*—The Chinese have a method of hatching the spawn of fish, and thus protecting it from those accidents which ordinarily destroy so large a portion of it. The fishermen collect with care on the margin and surface of waters all those gelatinous masses which contain the spawn of fish; after they have found a sufficient quantity they fill with it the shell of a fresh hen's egg, which they have previously emptied, stop up the hole, and put it under a sitting fowl. At the expiration of a certain number of days they break the shell in water warmed by the sun, the young fry are presently hatched, and are kept in pure fresh water till they are large enough to be thrown into the pond with the old fish. The sale of spawn for this purpose forms an important branch of trade in China.

5. *Natural Changes in Carrara Marble.*—Carrara marble presents, according to M. Ripetti, an instance of chemical changes in the colouring principles without any alteration in the carbonate of lime. The marble of Carrara does not always possess that brilliant whiteness for which it is so famed; it is for the most part of a greyish tint, and is of its utmost whiteness only in certain parts where veins have been formed, or else spots of oxide, sulphate, or sulphuret of iron. Some of these stains are old and fixed, but others seem to be of recent formation and are removed by water running over them, so that in a short time the marble becomes as white as snow. The workmen express this effect by saying, "The marble cleanses itself." Whole masses seem to change by a chemical process, and in support of this opinion, it has been observed that the marble of the ancient excavation of St. Silvestro, which was formerly of no value, has now become excessively white: and that in general the different species of Carrara marble vary with time, and become more and more pure.—*Gior. de Fisica.*

6. *Note on the existence of a Nitrate and a Salt of Potash in Cheltenham Water*, by M. Faraday, &c.—Having undertaken at the request of Dr. Creaser an examination of some water from Chelten-

ham, I had occasion to remark in it the existence of two substances not before observed in waters from that place; and though of no importance in a medicinal point of view, yet as relates to the sources from whence the waters obtain their impregnations, and to the illustration they afford of the use of two tests suggested by Dr. Wollaston but not very frequently, I believe, in the hands of chemists, they may I think possess interest; one of these substances is nitric acid, and the other potash.

The source from which the water was obtained is called, I believe, the Orchard Well. It had been some time in disuse, but has more lately been cleaned out and deepened, and is now about fifty-six feet to the bottom. The solid contents of a pint of this water examined in London were,

Carbonate of lime	1·6 gr.
Sulphate of lime	14·5
Sulphate of magnesia	12·4
Sulphate of soda	3·7
Muriate of soda	97·0
	129·2

Besides which, the water contained a portion of carbonic acid; and a small quantity of peroxide of iron had settled to the bottom of the bottle.

On adding sulphuric acid to a portion of this water in quantity abundantly sufficient to decompose all the salts subject to its action, and boiling such acidulated water in a Florence flask, with a leaf of gold for half an hour or an hour, the gold either in part or entirely disappeared, and a solution was obtained which when tested by proto-muriate of tin, gave a deep purple tint. Hence the presence of nitric acid, originally, in the water was inferred, and that no mistake might occur, a solution was made in pure water of all the salts except the nitrate found in the water, boiled with some of the same sulphuric acid, and tested by the same muriate of tin; but in this case no colour was afforded, or any gold dissolved.

The potash was ascertained to be present by evaporating a quantity of the water until reduced to a small portion, filtering it and then adding muriate of platina in solution. Three pints of water, evaporated until about one ounce of fluid remained, gave an abundant precipitate of the triple salt of potash and platina. In cases where small quantities of the water was tried, it was necessary to let the liquid stand an hour or two after applying the muriate of platina, but the triple salt always ultimately appeared.

Two pints of the water, evaporated to dryness in a silver crucible, gave on re-solution of the residuum a decided though very minute trace of silica.

7. *Iodine in Mineral Waters, &c.*—At p. 168 of the last volume of the Journal, was noticed the presence of iodine in the waters of Sales in Piedmont. It was discovered in them by M. Angelina. It appears that since then M. Krüger of Rostock, has found iodine in the mother liquor of the saline springs of Sülzer in Mecklenburgh-Schwerin, and M. Fuschs has found the same substance in the mother water of the Sal Gem of Hall in the Tyrol. It appears, however, that as yet the iodine has not really been separated from their mother liquors, but its presence has been ascertained by the blue colour given to starch dissolved in nitric acid, and there appears to be no doubts in the minds of the experimenters on the reality of its presence.—*Gior. de Fisica.*

8. *New Vesuvian Minerals.*—MM. Monticelli and Covelli mention the following minerals as having been sent forth to the surface of the earth during the eruption of Vesuvius in October, 1822. 1. Two small pieces of true lapis lazula found in the red sand sent forth on the 24th of October. 2. Several varieties of quartz, resinous quartz, and its passages into a lava composed of amphigene and pyroxene. 3. White and green phosphate of lime in fine hexaedral prisms and acicular crystals. 4. Perfect cubes of melilite, much larger than those of Capo di Bove. The two latter species were found in a current on the sides of Monte Somma above Pollena. 5. Gehlenite resembling that of Tassa. 6. Specula iron in brilliant plates above an inch wide. 7. Oxide of iron in octoëdrons, above half an inch in diameter; the same also in mammelated or fused masses. 8. Antimonial iron. 9. Glass of antimony apparently containing a small quantity of osmium.—*Bib. Univ.* xxv. 42.

9. *Products of Combustion of certain Coal Strata.*—In the neighbourhood of Aubin (Aveyron,) there exist certain coal strata, some of which are worked, and others are burning, having been on fire for thirty or forty years. It has been remarked as singular that no muriatic acid or ammonia occur in the products of this combustion: much sulphurous acid escapes, and various portions of sublimed sulphur, and acid aluminous efflorescences have been collected; but on chemically examining these and the other products obtained, neither muriatic acid nor ammonia have been observed. The coal nevertheless contains abundance of azote, and on distillation affords carbonate of ammonia.

10. *Advancement of the Ground.*—The inhabitants of the village of Hayotte, in the parish of Champlain, Canada, were alarmed on the 28th of August, 1823, by the motion of a large tract of land, containing a superficies of 207 arpents, It moved five or six arpents, (about three hundred and sixty yards) from the water's

edge, and precipitated into the river Champlain, overwhelming in its progress barns, houses, trees, &c. The river was dammed up for a distance of twenty-six arpents in an instant with an awful sound, and a dense vapour, as of pitch and sulphur. Various causes have been assigned for this phenomenon, of which the most probable, is the insinuation of water between the strata.—*Phil. Mag.* lxii. 470.

11. *Existence of Free Muriatic Acid in the Stomach.*—The following are the proofs of the existence of free muriatic acid which Dr. Prout has laid before the Royal Society. The contents of a stomach having been digested in distilled water, the solution obtained was divided into four equal parts. One of these evaporated to dryness, burnt and examined in the usual way, gave the quantity of muriatic acid in combination with fixed bases. A second being previously saturated with an alkali, was treated in a similar way, and gave the whole quantity of muriatic acid in the stomach. A third carefully neutralized with a known solution of alkali, gave the quantity of free acid. The fourth was reserved for any required experiment. In this way Dr. Prout ascertained that the unsaturated muriatic acid in the stomach was always considerable, and in one case twenty ounces of a fluid from a very deranged stomach, afforded him above half a drachm of muriatic acid of specific gravity 1.160.

12. *Use of Sulphate of Copper in Croup.*—Dr. H. Hoffman recommends the sulphate of copper as an excellent remedy in croup, especially after blood-letting. In slight cases he begins with giving from a quarter to half a grain every two hours; in those cases, however, where there is also laryngites, or bronchites, three, four, or more grains are administered, so as to excite instant vomiting; by so doing, the Dr. thinks that not only is the lymph expelled from the trachea, but also that the further secretion of it is prevented, so that the patient is very much relieved, and soon cured. After copious vomiting has been produced, the medicine is to be given in small doses, in conjunction with digitalis. In support of the utility of the above practice, Dr. H. affirms that he has employed it with the greatest success during a period of ten years, in a great number of children affected with croup, without ever having lost a patient in that time, notwithstanding the disease was often at its height when he was first called in.—*Med. Rep.* N.S. i. 85.

13. *On Sand-drigs or Fulgorites, by* MM. Fiedler and Hagen.—The ensuing observations have been selected from the account given in the *Bib. Universelle,* respecting these natural sand-tubes, by MM. Fiedler and Hagen. The latter person was particularly well circumstanced in ascertaining the cause of their formation. The

Report of Dr. Fiedler states, that being anxious to investigate the circumstances and formation of these tubes in the sandy districts of Austria, he passed over those parts from Vienna towards Hungary, and from thence to Stampfen, in search of them, and was ultimately fortunate enough to find one on the most elevated part of one of the hills, in the neighbourhood of Zankendorf, about a league north of Malaczka.

This tube was half an inch (of Leipzic) at the longest diameter at the upper part. On carefully removing the sand round it, commencing at some distance, it was found that at the depth of two ells a thin bed of quartz, in large grains, occurred, and immediately beneath that, a yellow plastic clay. The sand was now removed round the tube, and it was found that although above it formed an angle of about 80° with the horizon, yet it soon became vertical, and continued so to its lower extremity. It was probably at first some feet longer, but had been destroyed by the wind, for fragments were found lying about, which, however, could not be adapted to the upper end of the tube. At six inches from its upper extremity a small branch, about 4½ inches, passed off from it, and 32 inches lower the trunk was divided into two branches. The N. E. branch was 7½ inches long and terminated on the clay by a lengthened swelling, hollow within, its surface being composed of fused siliceous sand. In many places the course of the electric fluid could be perceived on the clay by the various red tints it had produced, which penetrated the clay to a depth of eight inches. The fusion appeared to have ceased when the electricity gained the clay. The S.E. branch was 1½ inches longer than the other; before arriving at the clay it passed close round one side of a piece of quartz, an inch in diameter, and was fused to it. It terminated on the clay just as the other branch did, the extremities being removed about 2½ inches from each other.

In many places the tube was contracted to a small diameter, in which places long bulbs had been formed, but the tube itself was hollow within to a considerable extent. It resembled exactly the tubes of the same kind found in the lands of Senner, and like them was surrounded with red sand*. Just beneath the point of separation lay a quartz pebble, but as it was entirely surrounded with sand, it probably had no influence in producing the division.

The following is from the account given by M. Hagen, of a tube formed by lightning, and which he examined soon after its formation. It occurred at the village of Rauschen, on the shores of the Baltic, in the province of Sarnlande. A storm occurred on July 17, 1823, which, about evening, approached the village; near seven o'clock the clouds descended towards a young birch-tree, about twelve feet high, and the lightning descended along its trunk

* The sand in the neighbourhood became red by heat.

and penetrated the soil at its foot. A man, who was standing at the door of a building, about fifty steps off, saw a juniper bush still burning under the tree, but it was soon extinguished by the rain. The neighbours immediately came together at the tree; they found two narrow deep holes in the earth, and affirmed that one of them was hot to the touch.

On examining the tree and place two or three days afterwards, slight traces of the passage of the lightning was found on the tree, and the juniper bushes and herbs in the neighbourhood of the tree, were generally charred. The holes in the earth, however, presented no signs of combustion, not even on the small roots which appeared on their inner surface. The soil was a coarse yellow sand, reposing, at the depth of two feet, on a bed of vegetable earth. On removing the sand, &c., it was observed that oneof the holes did not descend more than a foot, and offered nothing remarkable, but a little lower down, the commencement of a vitreous tube was found; the tube could not be removed whole, because of its fragility, but the fragments were collected, and it was found to have penetrated even into the vegetable earth, where, though many grains of sand had been agglutinated, they had not formed a regular tube. The fragments were covered with a black matter. The other aperture, which had been fonnd hot after the descent of the lightning, did not seem to be accompanied by, or terminate in any vitreous tube.

Some of the fragments withdrawn were three inches long, and all were distinguished from similar tubes or fragments from other places, by their thinness and fragility; they were scarcely as thick as paper, and were semi-transparent. The surrounding sand appeared blackened here and there; the interior of the tube was bright and shining from a thin coat of flux. It was of a pearl gray colour and beset with black points. The tubes were flattened, and extended on opposite sides in a zigzag direction. The sides of the tubes, in some parts, almost came together, but no branches were sent off, except where it had penetrated the vegetable earth, and at that part it became almost filamentous. The fragments together formed a length of above 21½ inches. On examination, the black powder appeared to be carbonaceous, for it resisted the action of acids, but disappeared before the blow-pipe.—*Bib. Univ.* xxiv. 106.

On the Recurrence of the Smallest Light of the Variable Star Algol. by W. M. Moseley, Esq.

Blackman Street, March 22, 1824.

My dear Sir,

If you can find a space for the accompanying communication, you will render a service to practical astronomy.

I remain, dear Sir,

Yours, very truly,

J. South.

To W. T. Brande, Esq.

Wintordyse House, Bewdley,
March, 20, 1824.

" Dear Sir,

" In compliance with your suggestion, I transmit you a table of the recurrence of the smallest light of Algol; the most remarkable of all the variable stars; Professor Wurm has inserted in Bode's Jahrbuch for 1822, p. 119, a table of this periodical change during the years 1820, 1821, and 1822, and what I now send, is a continuation for the ensuing summer, calculated from the data given in the professor's introductory dissertation. He informs us, that he had verified the period of the change of light, by comparing a recent observation of his own, on September, 23rd, 1813, with Mr. Goodrich's, of earliest date, or January 31st, 1783, after 4540 revolutions had occurred in the interval. The complete change occupies eight hours, or eight hours forty minutes; but this period is very difficult to ascertain accurately. The star when smallest, appears of the fourth magnitude; but when brightest, of the second. It seems, however, from Mr. Goodrich's observations, that when at a maximum, its brightness

is different at different times; and Mr. Pigot remarks, that it sometimes is *more luminous* than α Persei. It is convenient to place a bar in the eye-piece of the telescope, of such diameter as just to cover a star of the fourth magnitude; the enlargement will then be more distinctly perceived in its progress; but it will require considerable experience to mark the extremes satisfactorily. The table is reduced to *mean* time at Greenwich. The recurrence of the most diminished light is given to the nearest minute; and on those days only when the star may be distinctly seen at the expected hour.

1824

		H. M.
April	14	10.25
	17	7.14
May	7	8.57
	24	13.51
June	13	15.34
	16	12.23
	19	9.12
July	6	14. 6
	9	10.54
	26	15.48
	29	12.37
August	1	9.26
	17	14.20
	20	11. 9
	23	7.58
Sept.	7	16. 3
	10	12.52
	13	9.41
	16	6.30

"I beg you will dispose of this account as you think proper, and

I hope it will be found to correspond with the phenomenon. It is a tedious process to go through, but I hope I have made no mistake. I remain, dear Sir,

Yours, very respectfully,

W. M. Moseley.

" *To J. South, Esq.*

Note.

" To preclude all possibility of error in identifying the star, its right ascension is $2^h\ 57'$ and declination $40°\ 16'$ N."

ART. XV.—METEOROLOGICAL DIARY for the Months of December, 1823, January and February, 1824, kept at EARL SPENCER's Seat at Althorp. in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For December, 1823.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Monday	1	47	49	29.42	29.61	WbS	SW
Tuesday	2	39	48	29.38	29.30	SE	SSW
Wednesday	3	37	43	29.30	29.37	SW	SW
Thursday	4	40	44	29.05	29.30	W	W
Friday	5	35	42	29.56	29.60	W	W
Saturday	6	33	42	29.41	29.76	NE	NNW
Sunday	7	29	36	30.30	30.30	WbN	SW
Monday	8	31	43	30.26	30.13	WSW	WbS
Tuesday	9	33	44	30.19	30.19	NW	W
Wednesday	10	30	40	30.22	30.16	WbN	WSW
Thursday	11	38	47	29.95	29.88	WbS	SW
Friday	12	38	41	29.65	29.61	W	W
Saturday	13	33	38	29.50	28.98	W	NW
Sunday	14	26	38	30.10	30.06	W	WSW
Monday	15	35	41	30.08	30.12	W	W
Tuesday	16	30.5	45	30.00	29.79	SW	S
Wednesday	17	37	48	29.50	28.80	SW	S
Thursday	18	33	39	29.29	29.31	W	W
Friday	19	30	34	29.52	29.60	WNW	WbN
Saturday	20	29	34	29.14	29.00	SE	WbS
Sunday	21	32	41	29.13	29.20	SW	SW
Monday	22	38	41	29.50	29.68	NW	NW
Tuesday	23	33	44	29.63	29.65	SSW	WNW
Wednesday	24	41	48	29.80	29.65	SW	SW
Thursday	25	46	49	29.83	29.73	SW	SW
Friday	26	40	46	29.70	29.48	SW	SW
Saturday	27	42	44	29.10	29.11	SW	WSW
Sunday	28	36	40	29.35	29.19	SW	S
Monday	29	39	40	29.00	28.95	SW	WSW
Tuesday	30	39	45	28.60	29.06	WbS	W
Wednesday	31	30	45	29.35	29.54	W	W

For January, 1824.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Thursday	1	38	46	29.14	29.05	W	SW
Friday	2	44	45	29.20	29.60	WbS	WNW
Saturday	3	36	43	30.10	30.23	W	NW
Sunday	4	31.5	44	30.36	30.38	NW	NW
Monday	5	39	42	30.34	30.32	W	SW
Tuesday	6	35	40	30.13	30.11	SW	WbN
Wednesday	7	29	40	30.25	30.24	NNW	NNW
Thursday	8	30.5	39	30.20	30.13	SbE	SW
Friday	9	37	46	30.10	30.05	SW	SW
Saturday	10	41	48	30.00	29.97	SW	WNW
Sunday	11	35	39	30.29	30.28	N	NNE
Monday	12	26	35	30.33	30.33	W	SW
Tuesday	13	26	33	30.37	30.44	W	W
Wednesday	14	26	31	30.39	30.28	W	NNW
Thursday	15	30	36	30.30	30.30	WNW	NW
Friday	16	30	35	30.47	30.50	NW	WbN
Saturday	17	25	35	30.46	30.38	W	W
Sunday	18	33	40	30.32	30.28	W	W
Monday	19	34	41	30.12	30.06	NW	W
Tuesday	20	30	43	30.03	29.92	SW	WbN
Wednesday	21	38	44	29.53	29.41	SW	WSW
Thursday	22	35	42	29.19	28.90	W	S
Friday	23	38	44	28.70	28.80	WSW	NW
Saturday	24	34	47	29.53	29.69	W	W
Sunday	25	40	50	29.72	29.64	WbS	SW
Monday	26	47	52	29.56	29.90	SW	SW
Tuesday	27	42	50	29.67	29.41	SW	W
Wednesday	28	35	42	29.40	29.38	WbS	WbN
Thursday	29	34	41	29.70	29.40	WbN	NW
Friday	30	25.5	37	29.93	29.01	NW	S
Saturday	31	33	[illegible]	29.80	29.80	S	S

For February, 1824.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn	Eve
Sunday	1	28	42	29.79	29.71	E	SE
Monday	2	30	41	29.82	29.90	NE	SE
Tuesday	3	34	43	29.94	29.80	SE	SbW
Wednesday	4	38	47	29.53	29.57	SW	SW
Thursday	5	33	41	29.62	29.70	W	W
Friday	6	32	42	29.63	29.90	W	SW
Saturday	7	39	46	29.90	29.90	SW	SW
Sunday	8	41	54	29.98	30.10	SW	SW
Monday	9	43	51	30.18	30.29	SW	NW
Tuesday	10	35	50	30.25	30.06	NE	WbS
Wednesday	11	35	43	30.18	30.16	W	NW
Thursday	12	31	48	29.80	29.47	SW	SW
Friday	13	34	42	29.09	28.90	SW	SW
Saturday	14	33	39	28.84	29.00	NE	NE
Sunday	15	32	41	29.37	29.40	N	NNE
Monday	16	24.5	36	29.48	29.39	NE	ESE
Tuesday	17	27	38	29.30	29.27	SE	ESE
Wednesday	18	31	42	29.20	29.18	ESE	ESE
Thursday	19	34	45	29.28	29.28	ESE	ESE
Friday	20	37	40	29.38	29.48	E	E
Saturday	21	25	42	29.59	29.62	W	W
Sunday	22	32	45	29.74	29.74	E	E
Monday	23	37	41	29.80	29.83	E	
Tuesday	24	37	31	29.83	29.78	E	
Wednesday	25	33	36	29.70	29.70	NE	NE
Thursday	26	30	40	29.68	29.64	NbE	NE
Friday	27	33	34	29.55	29.57	NE	NE
Saturday	28	34	39	29.62	29.67	NE	NE
Sunday	29	33	37	29.77	29.78	NE	NE

CONTENTS

OF

THE QUARTERLY JOURNAL,

No. XXXIV.

TO OUR READERS AND CORRESPONDENTS.

Several Papers remain on our hands, which, for reasons already communicated to the respective authors, we are obliged to decline publishing. We shall be under the necessity of destroying these papers, unless they are immediately applied for.

We regret that the Paper on the Analysis of the Holy-well Water is too long for insertion.

Mr. Stevenson s Paper is declined in consequence of the number of engravings requisite for its illustration. The drawings shall be taken care of.

We suspect that our voluminous Correspondent upon the subject of London Bridge is not quite disinterested, and if he will favour us with his address, he shall be convinced that we have not neglected the ample consideration of his communications.

The request of E. A. has been made known in the proper quarter.

Mr. Bigsby's Paper has reached us, and will appear in our next Number.

We never advocated the Repeal of the Salt-Tax, and cannot therefore conscientiously insert the Letter of our Correspondent at Liverpool.

Mr. Walsh's Letter reached us too late for insertion.

We cannot meddle in the matter alluded to by our Correspondent at Edinburgh, who nevertheless has our thanks.

THE

QUARTERLY JOURNAL,

July, 1824.

ART. I. *On the Horary Oscillations of the Barometer.* *By* J. Frederic Daniell, F.R.S.

HAVING had occasion, some time ago, to inquire into the facts regarding the daily periodical fluctuations of the barometer, I was greatly struck, not only with the regularity of their occurrence, but with their gradual decrease in proceeding from the equator towards the poles, as shewn by the experiments of different observers. The following table constructed from the best authorities, places this circumstance in a striking point of view.

TABLE I.

Mean Periodical Movement of the Barometer, at different Latitudes.

Names of Places.	North Latitude.	Mean periodical movement of the Barometer.
St. Thomas'	0°.24	0.074 inch
Sierra Leone . . .	8.29	0.073
Trinidad	10.39	0.063
Jamaica	17.56	0.058
Clermont-Ferrand .	45·47	0.039
Paris	48.50	0.028
London	51.31	0.015

In endeavouring at the same time to account for these phenomena upon the known laws of aëriform fluids, I was led to construct an hypothesis which appeared to me to explain this gradual decrease of the oscillations; but at the same time pointed out a condition of the problem which would at once, if confirmed by experience, be a test of the correctness of the solution.

Let us suppose that in the atmosphere surrounding the earth a circulation is kept up between the poles and the equator; and that the cold dense air of the former regions flows in a lower current to the latter, while the elastic air of the latter is returned in an upper current to the former. There can be no difficulty in imagining further that, as long as these currents are maintained with regular velocities, a barometer, at all intermediate stations, might exhibit an equal pressure of the aërial columns; for as much air would flow from their summits as would be returned to their bases. A general alteration of temperature, which equally pervaded both currents, would produce no alteration in the weight of a vertical section, comprising both; nor would a partial alteration equally diffused through the upper and under section of any one column. The velocities of the currents would be partially altered thereby, but the higher and lower would still compensate each other. But an alteration of temperature which affected the upper and lower currents unequally, would produce partial expansions and contractions, which would effect an unequal distribution of the ponderable matter. If the lower stratum of any perpendicular section were expanded by heat, while the upper were unaffected, the outgoing current of that section would be increased, while the incoming current would be checked; and the balance of the two being disturbed, the total weight would be diminished. On the other hand, a local decrease of temperature would produce the analogous contrary effect. Now the alternations of heat and cold, produced by the changes of day and night, although they may be regarded in a general way as pervading both currents, act with sufficient inequality to induce us to expect a corresponding fluctuation in the weight of the atmosphere at any particular point. The heating surface being below, the warm particles quickly ascend, and

are immediately replaced by the cold particles from above; and by this circulation the diffusion of heat is very rapid. But the exchange of particles between the upper and lower strata must occupy some time, however small the interval, and the consequence must be that the barometer will measure by its fall the amount of the inequality. So on the other hand, in the process of cooling, in the absence of the sun, experiment has shewn that the lower strata of the air become more rapidly affected by radiation than the upper, and the total increase of weight from this cause, will be shewn by the rise of the mercurial column.

If we trace this effect along any given meridian, we shall become sensible of the manner in which this influence operates. Beginning at the equator, the only circumstance which we have to appreciate is the irregularity of the lateral expansion or contraction. As the earth acquires warmth from the sun, the barometer falls; but the check which the incoming current from the poles sustains, must be felt along the whole line of its course; and its due velocity being opposed, without any adequate compensation in the upper current, the barometer would have a tendency to rise at all latitudes between the equator and the pole. Assuming an intermediate station upon the same meridian, we should have the same effect produced by the unequal expansion of the lower current of the atmosphere, but opposed now by the impulse communicated from the equator. The fall of the barometer would only then represent the balance of the two effects, and must be less than at the equator. The further we proceed towards the pole, the more must this revulsive action accumulate, and the less must the balance of the two become, till at some neutral point they are exactly equal. Beyond this point, again, the former action would exceed the latter, and the barometer would rise in the higher latitudes, while it was falling in the lower.

The results of the preceding table obviously coincide with such a gradual progress towards a neutral point: but up to the time when I published an essay upon this subject, there were no experiments to prove the corresponding opposite effect beyond this limit. By a careful examination, with this view, of the meteorological register

kept at Melville Island by the expedition under the command of Captain Parry, I found that there was distinct evidence of the anticipated result. The barometer in that high latitude periodically rose at those hours when it is known to fall in the southern degrees. The following tables present the monthly means arranged in the proper order for exhibiting the conclusion.

In the first, including the winter half of the year, it will be observed that the mean temperature scarcely varied from noon to midnight: the effect of the remote equatorial expansion was therefore unopposed, and the barometer constantly rose from 6 A.M. to 6 P.M., in coincidence with the fall in the lower latitudes. From 6 P.M. to 6 A.M. it as constantly fell.

In the second, comprising that portion of the year when the sun was above the horizon, the daily variations of temperature were considerable, and the effect less regular, but nevertheless the barometer constantly rose from noon to 8 P.M., and then descended to midnight.

TABLE II.

Shewing the mean heights of the Barometer and Thermometer at four different hours of the Day at Melville Island.

1819	6 A. M.		Noon.		6 P. M.		Midnight.	
	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.	Bar.	Ther.
September . .	29.881	+21.5	29.906	+23.7	29.920	+22.7	29.890	+21.3
October	29.777	− 4	29.808	− 2.8	29.840	− 3.9	29.825	− 5
November . .	29.935	−21	29.946	−20.1	29.946	−20.1	29.937	−21.2
December . .	29.874	−23	29.872	−21	29.881	−21.1	29.893	−21.6
1820 January	30.010	−30.3	30.036	−30	30.068	−29.9	30.063	−30.4
February . . .	29.741	−32.8	29.758	−30.8	29.782	−32.6	29.771	−33.5
10 days of March	29.551	−19.1	29.561	−14.5	29.614	−18.5	29.571	−20.5
Means . . .	29.8288		29.8110		29.8611		29.8500	
Difference .	−.0212		+.0122		+.0231		−.0111	

TABLE III.

Shewing the Mean Heights of the Barometer and Thermometer at six different hours of the day at Melville Island.

1820	4 A.M.		8 A.M.		Noon.		4 P.M.		8 P.M.		Midnight.	
	Barometer	Thermometer	Barometer	Thermometer	Barometer	Thermometer	Barometer	Thermometer	Barometer	Thermometer	Barometer	Thermometer
20 days of March	29.894		29.885		29.880		29.902		29.906		29.910	
April	29.963		29.976	−9.2	29.971	−3.7	29.973		29.988	−8.1	29.987	−12.8
May	30.116		30.119	+ .15	30.099	+20.3	30.099		30.109	+18.2	30.109	+13.1
June	29.826		29.828	+36.3	29.821	+38.6	29.823		29.819	+36.5	29.817	+33.6
July	29.668		29.675	+42.5	29.674	+45	29.663		29.665	+42.7	29.660	+39.1
August	29.733		29.727	+32.7	29.734	+35.5	29.737		29.738	+32	29.735	+30.5
Mean	29.8666		29.8683		29.8631		29.8661		29.8708		29.8696	
Difference . .	−.0039		+.0017		−.0052		+.0030		+.0047		−.0012	

Upon the return of the last expedition from the northern coast of America, I was extremely anxious again to bring my hypothesis to the test of experience, and for this purpose was favoured upon application, with the loan of Captain Lyon's Meteorological Journal. This, as well as all other nautical registers which I have had an opportunity of examining, has been kept with the utmost precision and neatness; and it is highly gratifying to find so much attention to the interests of science amongst our naval officers, who have such opportunities of enlarging our acquaintance with the different climates of the globe. The periods of the day were almost as favourable as possible to the comparison, but the latitudes were not as far removed as that of Melville Island from the influence of variations of daily temperature. The following table presents the monthly means of the observations for two years, during which the Hecla was confined between the latitudes 66° and 70°:

TABLE IV.

Shewing the Mean Heights of the Barometer and Thermometer at four different Hours of the Day on board H. M. S. Hecla, between the Latitudes 66 *and* 70.

Date.	A. M. 4		A. M. 8	P. M. 4		P. M. 8
	Bar.	Ther.	Bar.	Bar.	Ther.	Bar.
1821						
August . .	29.835	33.5	29.846	29.818	39.9	29.825
September .	29.958	29.8	29.974	29.973	31.3	29.977
October . .	29.881	8.9	29.876	29.889	17.6	29.898
November .	30.166	2.7	30.156	30.165	12.6	30.159
December .	29.904	−19.2	29.898	29.914	−11.5	29 918
1822						
January . .	29.921	−26.9	29.924	29.933	−20	29.952
February . .	29.762	−27.5	29.716	29.753	−18.5	29.761
March . . .	29.849	−17	29.854	29.864	− 3.8	29.852
April . . .	29.895	− 0.2	29.893	29.907	+13.9	29.918
May . . .	29.985	+13.5	29.957	29.973	+31.5	29.978
June . . .	29.886	26.9	29.877	29.897	38.2	29.868
July . . .	29.682	32.7	29.693	29.694	40.6	29.702
August . .	29.613	31.5	29.636	29.661	36.5	29.667
September .	29.883	22.2	29 883	29.895	28.3	29.894
October . .	29.967	10.7	29.981	29.981	18.1	29.985
November .	29.875	−22.6	29.876	29.884	−13.4	29.882
December .	29.756	−32.5	29.739	29.741	−25.1	29.726
1823						
January . .	29.877	−20.2	29.902	29.898	−10.6	29.893
February . .	29.904	−21.9	29.906	29.905	−13.4	29.907
March . . .	30 050	−21 1	30.055	30.050	−12	30.061
April . . .	29.957	− 9	29.956	29.957	+ 7.5	29.954
May . . .	29.929	+16.9	29.916	29.920	33.3	29.921
June . . .	29.922	23.4	29.910	29.909	41.2	29.909
July . . .	29.507	33.2	29.499	29.609	43.8	29.608
Mean . . .	29.874		29.872	29.880		29.879
Difference	−.005		−.002	+.008		−.001

It appears from this table that the rise in the mercurial column from 8 A.M. to 4 P.M. was nearly constant, and upon further examination it will be found that in the only two exceptions of any amount, namely the months of January and March 1823, some unusual influence prevailed in the atmosphere. The first was distinguished by an unusually high mean temperature, and frequent storms of wind. Captain Parry remarks in his Journal, "from the morning of the 24th till midnight on the 26th, the mercury in the barometer was never below 30.32 inches, and at noon on the latter day had reached 30.52 inches, which was the highest we had yet observed it in the course of this voyage. This unusual indication of the barometer was followed by hard gales on the 27th and 28th, first from the S.W., and afterwards from the N.W., the mercury falling from 30.51 inches at 8 P.M. on the 26th, to 30.25, about 5 P.M. on the 27th, or about 0.26 of an inch in nine hours before the breeze came on. At midnight on the 27th it had reached 29.30, and on the following night 29.05, which was its minimum indication during the gale. These high winds were accompanied by a rise in the thermometer very unusual at this season of the year, the temperature continuing above 0° for several hours, and very near this point of the scale for the whole two days."

The month of March, on the contrary, was as much below the mean in temperature, as January was above it, and the observation renders it probable that the usual course of the season was modified by some extraneous cause.

I am aware that it may be objected, that these observations were not made with all the precision that the accurate determination of such small quantities requires, and particularly that the heights of the barometer were not corrected for the variations of temperature. The objection, to some extent, is certainly valid, and it is much to be lamented that the advantages of the utmost attainable degree of precision in these observations have not hitherto been duly appreciated: but when it is recollected that the instrument made use of was placed in the cabin of the ship, where considerable pains were taken to maintain an equal temperature, it

will be found that less importance attaches to the omission in this particular instance than might at first be supposed. In the last voyage, more especially, the precautions which were adopted to secure this important end, were eminently successful. It appears, for instance, by Captain Parry's register, that in the months of October and November, the mean temperature of the external air varied 32°, while that of the air of the lower deck only varied 5°, so that the changes in the course of the 24 hours could have been scarcely appreciable. The return of the various expeditions which are now about to depart once more for the Arctic Regions, the officers of which have most zealously undertaken to make the observations with all the requisite precautions, will, it is to be hoped, set this interesting question at rest, and not only determine the existence of the phenomenon which I have ventured to anticipate, but also the exact amount of the fluctuation.

I would here willingly have entered into some speculations upon the mean height of the barometer as shewn by the registers of the high latitudes, and which appear, upon the first view of the subject, to be considerably below those of the more southern regions, but doubts respecting the construction of the instruments destroy the necessary confidence in the observations. These doubts are more strongly than ever impressed upon my mind by the inspection of eight barometers which were prepared for the expedition which has just sailed from the river, by one of the first opticians in London, and who undertook to bestow unusual pains in their construction. No two of them agreed in height, and the greatest difference was full 0.2 of an inch. One standard barometer, however, now accompanies them, and may serve to determine the errors of the others, so that little doubt exists that we shall at length be able to arrive at some precise conclusions respecting the fluctuations of the atmosphere in the most interesting and inaccessible climate of the northern hemisphere.

The advantages to be derived from a proper attention to the construction of the barometer cannot be better exemplified than by the circumstance of the same instrument-maker having since

completed five barometers of very different capacities and diameters, whose difference from the mean and from the standard, with all corrections made, was only .006 of an inch.

ART. II. *On the Alterations of Rate produced in Chronometers by the influence of Magnetism. By* George Harvey, Esq., F.R.S.E., *&c.*

THE power which a magnetic force possesses, of accelerating the rate of a time-keeper in some situations, and of retarding it in others, is a fact which has been verified by many interesting and important experiments. It is singular, however, that the same attractive power, which when applied in different directions to one chronometer, tends either to accelerate or retard its rate, should in another, when allowed to operate under the same conditions, as to intensity and position, produce results precisely the reverse. It will be the object of the following paper, to refer these apparent anomalies, to the *varieties of imperfect isochronism*, existing among different chronometers.

To illustrate this view of the subject, suppose the balance of a chronometer in its quiescent position, having its thermometer-pieces in an active, but opposite state of polarity; and let the line joining those pieces, and which therefore passes through the centre of the balance, denote the direction in which the magnetic force acts. Now since the thermometer-pieces possess opposite polarities, let that portion of the attractive influence which is destined to operate on the time-keeper, and is of an opposite kind to the polarity of the thermometer-piece on which it first acts, be allowed to exert its energy, the moment the oscillations of the balance take place. The effect of such an application will be, a decrease in the arc of vibration, in consequence of the effort made by the thermometer-piece, on which the magnetic force acts, to approach the attracting pole. This alteration in the amplitude of the arc of vibration, will therefore occasion some variation of rate in the timekeeper. If instead of supposing the attractive power to pass im-

mediately through the thermometer-pieces, it be allowed to produce its effects, on either side the point of quiescence, within certain limits, the arc of vibration will still be diminished, but in a less ratio than before; and changes of rate proportional to the variation of amplitude, will be the result.

Suppose in the next place, the direction of the magnetic force to pass through the centre of the balance, and the limit of the semi-arc of vibration; it is manifest, when motion is communicated to the balance, its effect will be to increase the arc of vibration, both from its attracting one of the thermometer-pieces, and repelling the other; and that therefore an alteration of rate, entirely the reverse of the former, will be the necessary result. It is also evident, that if the same attracting pole be applied on either side of its last-mentioned position, within certain limits, the arc of vibration will still be increased, but in a less ratio than before; and alterations of rate of the same kind, but of a less remarkable degree, will be produced.

If the time-screws are supposed to be magnetic, and the thermometer-pieces free from the attractive influence, similar results will take place.

Conceive in the next place, that in addition to the magnetism of the thermometer-pieces, the entire arcs of compensation possess also a property of the same kind; one half of each having northern polarity, and the other half southern; then will the time-screw attached to the arc of compensation, whose thermometer-piece has northern polarity, become a south pole; and that attached to the arc, whose thermometer-piece has southern polarity, a north pole; the transverse arm connecting the two, if of steel, presenting all the properties of a perfect magnet.

In this point of view, the entire balance may be regarded as a species of compound magnet, having two pair of opposite poles; and different phenomena will be exhibited, according to the direction in which the magnetic force acts. If, for example, the magnetic power be allowed to develop its influence, in a direction equally remote from the opposite poles of each of the arcs of compensation; and that we moreover suppose each pole to possess the

same degree of intensity, the acceleration or retardation produced by the action of the exciting force on one of the poles, will be entirely neutralized, by the opposite effects of the other. But if the attracting power be allowed to operate in a position nearer to one pole than the other, an alteration of rate will result from the change in the arc of vibration, necessarily produced by the inequality of action. If on the other hand, the magnetic force be applied, in a direction between the thermometer-piece and time-screw belonging to the separate arcs of compensation, and having a polarity of a contrary kind to them, a constant effort will be made by the balance, to accommodate its arcs of vibration to the united effect produced by the maintaining power, and the intensity of the magnetic action; and a similar tendency will likewise be displayed by the balance, if the disturbing pole be placed in the vicinity of a thermometer-piece and time-screw, having the same kind of polarity with it.

From the same considerations we may also infer, why a chronometer, having a balance powerfully magnetic, should present variations of rate, from the influence of the earth alone, according as it is situated with respect to the magnetic meridian. If the thermometer-pieces alone are magnetic, and the line joining them be placed in any other direction than that of the magnetic meridian, a continual effort will be made by the balance to regain this position, thereby producing a change in the arc of vibration. If also, the entire balance be considered as magnetic, some line may be found passing through its centre, in which it would repose in the direction of the magnetic meridian, if detached from the other chronometrical parts, and freely suspended. Hence it follows, that the moment the timekeeper is so placed, as to remove the balance from the last-mentioned position, a tendency will be created in it, to return to that state; and which, by producing variations in the arc of vibration, must at the same time be accompanied by sensible alterations of rate.

Having made these general observations, I shall finally consider the cause, why *similar* changes in the arc of vibration, should be frequently attended, in different chronometers, with *opposite* alterations of rate.

It may be questioned, if ever a chronometer existed, in which the vibrations of the balance were perfectly isochronous; or in other words, in which the adjustments of the spiral spring were such as to admit of its elastic force, varying precisely with the arcs of vibration. Mr. Atwood has shewn in the *Philosophical Transactions* for 1794, that although the relation between the elastic force of the spring, and the magnitudes of the arcs of vibration, may *appear* to be in a *perfect* ratio of equality, there may nevertheless exist such exceedingly minute deviations from this state, as to render it impossible to be detected, by the most delicate experimenter; and yet these minute inequalities may be considerable enough to produce in the interval of twenty-four hours, a sensible alteration of rate. Hence it is, that the application of a magnetic force to a chronometer, having a balance in any degree magnetic, in almost every case, produces a visible alteration of rate. In an example furnished by the able mathematician before quoted he demonstrates, that a variation of a thousandth part from a perfect state of equality, in the relation between the elastic force of the spring, and the arcs of vibration, is capable of producing an acceleration of $+ 2''.62$ in the daily rate, when the semi-arc of vibration is diminished 8°; and he even states, that an increase of rate amounting to 20 or 30 seconds may exist, and yet the differences arising from the deviation of the elastic force of the spring, from the law of isochronism, be too minute to be rendered sensible by any statical counterpoise of the force of the spring.

Assuming therefore a perfect isochronism in the vibrations of a balance, as a condition scarcely to be obtained, the deviations from it, may be contemplated under two points of view; since the elastic force of the spring may vary either in a *less* ratio than the angular distances from the point of quiescence, or in a *greater*; and which suppositions will account for the apparent anomalies presented by different chronometers, when subject to the action of a magnetic force.

This will appear evident, by referring to the function, which according to Mr. Atwood, represents the daily abberration of a time-

keeper, when the magnitude of the arc of vibration is changed, and which is

$$24^{h}\left\{\left(\frac{a}{a'}\right)^{\frac{1-n}{2}} - 1\right\};$$

where a denotes the primitive arc of vibration, a' that produced by the action of a disturbing force; and which, according to the direction of its action, may be either greater or less than a; and n the exponent dependent on the peculiar ratio existing between the elastic force of the spring, and the angular distances from the point of quiescence.

If we suppose the primitive arc constant, and the other elements a' and n of the formula variable, the entire function, as Mr. Atwood observes, will be susceptible of different modifications. Suppose, for example, we attribute to n a *less* value than unity *, a condition which corresponds to that of the elastic force of the spring, varying in a *less* ratio than the angular distances from the point of quiescence; it is manifest, that different values will be communicated to the function, according to the value assumed for a. If the supposition alluded to in an early part of the paper, of the attracting force passing through the thermometer-pieces be referred to, and in which the arc of vibration would be shortened by its operation, the value of a' must necessarily become *less* than a; and a *positive* value being thus communicated to the function, the time-keeper will *gain*.

In the next place, if the attracting force be conceived, as in the second supposition, to pass through the centre of the balance and the limit of the semi-arc of vibration, and which application will necessarily occasion a' to become *greater* than a, the numerical value of the formula will be *negative*, and the chronometer will *lose*.

If again we suppose n to be greater than unity, or the elastic force of the spring to vary in a greater ratio than that of the distances from the point of quiescence, the first of the preceding suppositions with respect to a', will give to the function a *negative* value, indicating a *retardation* of rate in the time-keeper.

* If we suppose $n = 1$, the whole function will vanish, in indicating a perfect isochronism; so that whether the arcs of vibration be increased or diminished by the action of a disturbing force, no alteration of rate will take place.

In like manner, by referring to the case in which a' is greater than a, the numerical value of the function will assume a *positive* character, and the chronometer will *gain*.

Thus, with changes in the amplitude of the arc of vibration, from less to greater, or from greater to less, resulting from the application of a disturbing force in different directions, will results entirely opposite in their character be produced in different chronometers, in consequence of *Varieties of Imperfect Isochronism*.

Plymouth, May 20, 1824.

ART. III. *On Indistinctness of Vision caused by the presence of False Light in Optical Instruments, and on its Remedies. By* C. R. Goring, M. D.

[Continued from p. 28.]

MICROSCOPES.—These instruments though but toys compared with telescopes, nevertheless deserve to be rendered as perfect as possible, for they yield not to them in the quantity and variety of rational amusement which they are capable of introducing us to (though not of the sublime description of the wonders of the heavens). Compound microscopes though not so much to be depended upon for the purposes of discovery and philosophical investigation as single lenses, are still best adapted for recreation, but all those which I have ever seen constructed on the common principle, are so full of fog as to be quite disagreeable for examining opaque objects, which render this defect more striking than transparent ones. This false light results from the custom of making the object-glass of a very small aperture, instead of giving it a larger one, and placing a stop in its proper place (the focus of the lens employed). It is totally impossible to get rid of the fog in any other way. No doubt the larger the aperture of the lens of the common object-glass, the more indistinctness is sensible; and the more it is reduced, the less;—but no practicable contraction of the aperture will effect the desired purpose completely,

because the principle itself is intrinsically bad, and incorrect at least for low powers.

Now, if we form a microscopic object-glass of a single lens of considerable aperture, having a stop in its focus of about the same diameter as the apertures of the common lenses used for compound microscopes, (that is to say, about one-tenth or one-twelfth part of their focal distance,) we shall form an object-glass which gives a clear image, free from fog indeed, but very deficient in other respects; for the stop being placed *where the rays cross each other*, a large portion of the aperture of the lens is called into action, in comparison to what is usually made use of, when it is at once limited by a stop of the same diameter applied close to the glass; the aberrations both chromatic and spherical are here immediately felt—to remedy these, another lens must be employed, the best position for which is close to or very near the farther side of the stop. The focus of it must be to that of the first as 3 to 2, or as 2 to 1—for low powers, however, it may be about $2\frac{1}{2}$ to 2—for the higher the best proportions seem to me to be as 2 to 1 *. The lenses employed should be plane convex, having their curves towards each other as represented in Figs. III, IV, V, and VI, Plate II. which are drawings of four object-glasses of this description which I have caused to be executed the lowest power is 2 inches focus, the highest $\frac{1}{2}$ an inch—the foci of the lenses, and the size of the stops, *&c.*, are as there represented †. These object-glasses I can

* The addition of this second lens has another good effect, for it enables us to regulate the compound focus so as to have the object as near to the object-glass as will consist with the distance which must be allowed for suffering the rays from a lens or mirror to fall upon it for the purpose of illumination when opaque,—for the light of opaque bodies diminishes according to the square of their distance, and thus the farther the object-glass recedes from them, the less light it receives. With transparent subjects, however, the case is different, at least when they are illuminated by the converging rays of a lens or concave mirror; for, by making the focus of this fall not upon the object *but upon the object-glass*, the maximum of light is obtained at whatever distance the object may be from the glass, so that the benefit of having them near each other is not so much felt as in the former case—the proportions I have recommended will answer every purpose.

† It will be obvious that a microscope of my construction may be used as

confidently recommend as greatly superior to those in common use; they are bright, clear, and distinct, free from spherical aberration, and will shew no sensible colour with opaque objects of any kind, not even with so trying a one as the enamelled white letters on a black ground generally used by opticians to try their telescopes with. When, however, they are made to view an object illuminated from behind, which does not suffer the light to pass through it while its edges are seen, as for example the legs of some insects, some kinds of moss, &c., which have very little transparency, the uncorrected colour is then decidedly seen—such objects are the best tests of achromaticity for telescopes as well as microscopes; equivalent terrestrial ones for a telescope will be the bars of a window seen from the interior of the apartment to which it belongs, or the naked branches of a tree in winter, seen against the light of the sky, more especially of the sun, and nearly opposite the observer. In addition to the four object-

a magnifier for a telescope. In fact it is in its principle nothing but the four glass erecting achromatic eye-piece of a day telescope a little modified (there is alas nothing new under the sun). Indeed, many of Mr. Tulley's astronomical telescopes are so constructed that the night eye-pieces can be applied to magnify the erected image formed by the two glasses, which do the work of my object-glass. It would, however, be much better, instead of increasing the depth of the eye-glasses in this case, to augment that of the erecting part, as a much sharper image is in this way obtained. There certainly are many objects which are seen better with this kind of eye-glass, such as Venus, and many double stars;—the number of refractions arrest a portion of the false light or halo which so commonly surrounds these objects. However, the same or nearly the same effects seem to be produced, by diminishing the aperture of the object-glass of the telescope, except that this seems to increase the spurious disc of the fixed stars, which the other method does not. Many suppose that great advantages are to be gained by making a microscope with a long tube, and a shallow eye-glass. I have satisfied myself repeatedly by experiment, that whether the required magnifying power is obtained in this way, or by a short tube with a deep eye-glass, the effect is precisely the same. The body of my microscope is seven inches long, having an achromatic eye-piece of about one inch negative focus, just like those applied to telescopes. I do not like the double and triple eye-glasses very commonly applied to microscopes, as they are apt to give double images, with luminous transparent objects.

glasses I have described, I have two more of $\frac{1}{4}$ and $\frac{1}{8}$ of an inch focus, which I have not inserted, because (though executed with the utmost care,) they are no better than the common ones. I was grievously disappointed with these, for I had fully expected that the same principle applied to deep object-glasses would form as superior an object-glass for high powers as for low ones: however the reverse is the fact;—it is one of those things which can only be learnt from experience, and could not have been predicated *à priori*. There is doubtless a reason for this, but I am not able to shew what it is. Still, therefore, the common object-glass is the best for high powers, *viz.*, for those of a quarter of an inch focus, and upwards. My object-glasses are however deep enough for all ordinary objects—certainly for all opaque ones. There are, nevertheless, many transparent objects which cannot be seen without object-glasses of *at least* $\frac{1}{10}$ inch focus,—such are many kinds of animalcules and the minute lines on the dust of a butterfly's wing, *&c.* For these the common single lens of small aperture will perhaps ever remain the only efficient object-glass,—an equivalent power obtained with my object-glasses, or those of the common construction of similar focus, by increasing the depth of the eye-glass will never shew the objects in question, because what may be called *the penetrating power* of a compound microscope depends upon the *depth* of its object-glass, as that of a telescope upon the *aperture* of the metal or glass which forms the image viewed by the eye-glass. The eye-glass either of a microscope or telescope merely developes what is contained in the image it enables us to view; it cannot of course render any thing sensible to our sight which does not exist in the spectrum formed by the object-glass or metal. I may here mention that I had previously constructed my microscope with one object-glass only of one inch focus, and got my powers by increasing the depth of the eye-glass as is done in telescopes. I however, found, that a large image viewed by a shallow eye-glass made a much better instrument than a comparatively small one (formed by a shallow object-glass), viewed by a deep eye-glass; indeed the same position holds good with regard to telescopes also, for the largest and longest (*cæteris*

paribus) are sure to be the best, because the image of such needs but to be little magnified to procure a given power, and it must be evident that the more an image is magnified the more its imperfections will become sensible, for no image can be free from imperfection like the object from whence it is derived.

I shall here advert to a circumstance (though rather foreign to my subject,) relative to the proper apertures of the common microscopic object-glasses, which is, perhaps, not duly attended to. It is certain that the more their apertures are reduced (within a certain point,) the more fog you exclude; and in this way you improve the instrument,—yet if this reduction is pushed too far, it will prevent you from seeing a certain class of objects, even while the vision of others seems to be ameliorated. Thus the parallel lines on the dust or feathers of a butterfly's wing can be just seen with an object-glass of $\frac{1}{10}$ inch focus, and $\frac{1}{25}$ inch aperture as nearly as it can be measured: if, however, this aperture is very slightly contracted, they can no longer be seen with any art or management of the light,—at the same time other objects will appear foggy and indistinct with this same aperture, especially if opaque, and the vision of them will be improved by diminishing it. I am disposed, therefore, to think that the apertures should be regulated by this ratio of $\frac{1}{25}$ inch aperture to $\frac{1}{10}$ inch focus *.

* The great Sir W. Herschell has condescended to notice this subject, without however determining precisely what the aperture of a microscope object should be, in his paper in Vol. LXXVI, p. 500, of the Transactions *of the Royal Society*.—" Investigation of the Cause of that Indistinctness of Vision, which has been ascribed to the smallness of the Optic Pencils." I think, however, it will be found that Sir W. had not obtained pencils of rays of such extreme smallness as he supposed from a calculation of what the size of the pencil *should have been*, according to the powers he obtained, for *the power of a compound microscope cannot be measured in the same manner as that of a telescope, by comparing the size of the ultimate pencil of rays after it has passed the eye-glass with the diameter of the object-glass or metal.* Had Sir W. actually measured the pencils with a dynameter instead of calculating their dimension, he would have found them much larger than he supposed. In fact, all we obtain from comparing the size of the pencil of rays which enters the eye with the diameter of the object-glass in a microscope, is *what the power of a tele-*

I have several much deeper made on this plan up to $\frac{1}{60}$ inch—all of which shew the parallel lines in question, and other equally difficult objects,—the deepest lenses have their apertures somewhat larger than this ratio, for the sake of the light, (for it appears that you may increase this aperture, though you must not diminish it, and yet see these objects, though the fog then becomes very great and disagreeable; the colour also grows very apparent on account of the largeness of the aperture relative to its focus. Common microscopic object-glasses as we all know are sufficiently achromatic with the small apertures, and the shallow eye-glass of one inch focus usually employed, in which respect there is an analogy between them and telescopes with object-glasses composed of single lenses of small aperture, and a shallow eye-glass. The achromatics only differ from them in carrying a larger aperture with a deeper eye-glass, which again have their limits, beyond which the colour appears as before.

In Figs. VII, VIII, and IX, are representations of some silver cups for holding very deep single lenses intended to view opaque objects, which, together with the object-glasses before-mentioned, were executed for me by Mr. Tuther, optician, in High Holborn, to whose politeness and skill I am indebted for being able to carry my intentions into effect. It is generally supposed that single lenses will shew objects perfectly clear and without fog, but this is not the case unless their apertures are very small,—lenses of $\frac{1}{30}$, $\frac{1}{40}$, $\frac{1}{50}$, and $\frac{1}{60}$th of an inch focus require their apertures to be so much reduced to shew opaque objects clearly, that it is scarcely possible to see at all with them from the want of light. These cups were contrived to remedy this defect as far as it is practi-

scope would be, having an object-glass of the same aperture with the microscopic one with a focal length, equivalent to the distance between the object-glass of the microscope, and the focus of its eye-glass having its image magnified by the said eye-glass. For example, I measured the power of a microscope in the legitimate way with two similar micrometers, one on the stage, the other at the field bar in the focus of the eye-glass—supposing the eye-glass of 1 inch focus to have magnified six times, the power was 36, while the size of the pencil at the eye-glass compared with the diameter of the object-glass was merely as 2 to 6—the one being $\frac{2}{100}$ of an inch, the other $\frac{6}{100}$.

cable;—their radius is only $\frac{1}{4}$ of an inch, their focus consequently $\frac{1}{8}$. These condense light much more than the larger cups commonly used, and illuminate much more powerfully. It is true that they only enlighten a small portion of an object, but then we can only see a very small portion with such deep lenses as they are intended to hold; they are not so small but that they may be made to receive and condense the whole of the light proceeding from a bull's-eye lens placed at a proper distance from them, and in this way with no other light than that of a common candle, I have been enabled to see well an opaque object with a compound microscope, having an object-glass of only $\frac{1}{30}$ inch focus set in one of them, with only a moderate aperture. A lens of $\frac{1}{60}$ inch set in this manner, used as a single lens, likewise shews opaque objects in a manner which leaves nothing to be desired.

I must mention, however, that it is necessary for the stops between which the lenses are placed to be very accurately made. They should be turned out of a piece of solid brass, the external one very thin, and the holes so correct as always to coincide with each other when the stops are turned round; the apertures must be quite free from burrs; in addition to which the stops must be so adjusted that the focus of the lens and that of the cup must precisely correspond, otherwise the benefit of the cup is in a great measure lost. Fig. IX will carry $\frac{1}{8}$ or $\frac{1}{10}$ of an inch without any stop at all, which is a great convenience, for the lens is in this case close to the eye, and the field of view larger in consequence: the stops for the deeper lenses are much shorter than they would be with larger cups, (Figs. VII and VIII,) so the field is increased in the same way, and the eye much less strained in using them than it would be were the lens farther off from it. I have shewn many individuals objects with the $\frac{1}{60}$ inch lens not remarkable for the strength of their eyes, who saw with perfect ease, and were not at all conscious of the extreme smallness and depth of the lens they were using. As single lenses are generally considered to be most adapted for making discoveries in natural history, as being less likely to create optical deceptions than compound magnifiers, I imagine I am doing naturalists a service in putting them into a

way of using very deep ones without destroying their eyes*. I humbly recommend the contents of this paper to opticians, without being at all ambitious to acquire the honour of teaching them their own profession. I have the highest consideration for their practical knowledge, and conceive that one ounce of it is worth a ton weight of that of a mere theorist; at the same time I hope they will accept of my apologies for pointing out a few circumstances to them, (certainly not of much importance,) which the value of their time and the multiplicity of avocations of higher consequence will not usually permit them to attend to. If what I have written shall prove of no service to *them*, it is quite clear that my labours have been utterly useless. Indeed, it is too much the case that the researches of amateurs only terminate in discovering something which was perfectly well known before, and which only therefore serves to shew their own shallow acquaintance with the subject, or in bringing forward something as an improvement which has been tried and rejected long ago by those practically versed in the mysteries of optics.

ART. IV.—*Hints on the possibility of changing the Residence of certain Fishes from salt water to fresh.*—By I. MAC CULLOCH, M. D., F. R. S., &c.

IN the tenth volume of *Tilloch's Journal*, there is a paper on the means to be employed for multiplying fish, translated from one which appeared in the *Moniteur*, by Monsieur Nouel, of Rouen. Although the chief speculations of this writer, which are of a very

* They may, perhaps, also thank me for informing them that Mr. Cornelius Varley, of Upper Thornhaugh-street, Bedford-square, (the inventor of the graphic telescope,) worked the small lenses for me which I have described; they were polished on wax tools, the figure is as correct as that of any shallow lenses, and their image will bear magnifying perfectly well. Mr. Varley and Mr. William Tulley of Islington, are the only individuals I know who can make such deep lenses as they ought to be made.

interesting nature, concern the means of transferring the inhabitants of fresh waters in one country, or those of certain lakes or rivers, to others where they are not found, some hints are also introduced respecting the possibility of rendering certain sea-fish inhabitants of fresh waters. The whole paper is highly worthy of attention; but I am not aware that it has been followed by any of the practical trials recommended by the author, on which its economical value must ultimately depend. An example in point which recently came under my notice in Shetland, has induced me to examine the subject with somewhat more care than the author of that memoir seems to have bestowed on it, and to inquire more minutely into the arguments on which the probability of success rests. The following seem to be the only results which have been obtained, or were previously known with respect to that part of M. Nouel's plan, which relates to the cultivation of sea-fish in fresh water.

The plaice, *Pleuronectes Platessa*, as it appears, has been carried from the North sea to the ponds of East Friesland, where it has become established. The herring is said by Liancourt to frequent the Potowmack, Hudson, Elk, and Delaware rivers; but it has not appeared that the author's project to take it from the Seine into fresh-water ponds has been put into practice. The authority of Twiss for the existence of this fish in the fresh water lakes of Ireland, is more than questionable, and M. Nouel is assuredly misinformed when he states that it is found in prodigious shoals in Loch Lomond and Loch Eck in Scotland, both of them fresh inland lakes. I know not how this author can have thus been misled, unless he has mistaken some of the sea lochs for fresh-water lakes; though he could scarcely have confounded those he has named with any of the western inlets. I shall hereafter, however, point out a fact which renders his assertion possible; though he could not have been acquainted with it, as it is not very long since it was known, and has not been published in any work likely to have reached his hands.

It is also asserted in the same paper, that the salmon, in Scotland, has, in certain lakes, become naturalized, " abandoning their erratic taste, for a calm and settled life." Whether such an experiment

might not succeed, by forcibly transporting the salmon to lakes from which they could not reach the sea, is yet to be tried; but certainly there are not at present any salmon found in the Scottish lakes, except where they have the power of making their annual migrations into salt water. That salmon are attached to the particular rivers where they have been spawned and bred, is believed by all the fishermen; but this does not prove that they are naturalised to those fresh waters, as they invariably return to the sea after having deposited their spawn.

According to Pallas, the sturgeon, the sterlet, and some species of salmon reside in the river Kama without ever descending to the Caspian sea; and the authority of such a naturalist is perhaps sufficient to establish this interesting fact.

These, then, are the whole of the proofs which, in M. Nouel's paper, are adduced in support of this project; it remains to be seen by what other facts and reasonings its plausibility may be supported, and an inducement offered to those who have it in their power, to make such experiments as alone can establish it among those facts in natural history which are capable of being applied to the uses of man; to increasing the quantity, or adding to the accessible variety of his food.

In the first place, it must be remarked, that the habits of many sea-fish are, in this respect, so convertible, or so easily assimilated to the requisite change, that a large portion of their time is passed in fresh water. The common salmon, the grey salmon, and the salmon trout, Salmo Salar, Salmo Eriox, and Salmo Trutta, are familiarly known to frequent rivers for the purpose of spawning; returning to the sea when this operation has been performed. The Salmo Migratorius leaves the lake Baikal for the same season; and, with us, the S. Lavaretus, or Gwiniad, and the S. Eperlanus or smelt, also quit the sea; ascending rivers at the spawning season, as does the Salmo Autumnalis, an inhabitant of the frozen ocean.

Now though M. Nouel is wrong in saying that the salmon is found in the Scottish lakes excluded from access to the sea, it is a fact that the salmon trout, or sea trout, as it is called in Scotland, is now a permanent resident in a fresh-water lake in the

island of Lismore, and without the power of leaving it or reaching the sea. There, it has been known for a long course of years, perfectly reconciled to its prison, and propagating without any apparent difficulty. If this fish, whose annual necessity for returning to the sea is the same as that of the common salmon, has thus easily become naturalized, there is little reason to doubt that the same experiment would succeed with the salmon itself. The fishermen object to that opinion, that this fish becomes meagre and diseased by its residence in fresh waters, and is compelled to go to the sea to recover itself. But we need not feel much concern respecting their philosophy; while they forget at the same time that it is the operation of spawning by which the fish is injured, and that this consequence happens alike to sea-fish, from the same causes. It remains to be proved that the salmon would not recover itself in fresh-waters, as the sea trout does in Lismore; and this is the experiment which is to be tried before we are entitled to pronounce a negative. To render the salmon a permanent resident of the fresh-water lakes of Scotland, would unquestionably be a great gain; and that this has not been tried, often as it has been urged on those who have the means, is only an additional proof of the plodding incredulity and obstinacy of those who are averse to all innovation because it is innovation, and who believe that they have themselves attained the summit of all possible knowledge.

With respect to the smelt, its delicacy would render it a very desirable acquisition in our ponds, while its size would probably cause it to find an easy supply of food, and its facility of living for a time in fresh water render its naturalization easy. I accordingly caused some trials to be made for this purpose: they did not however succeed, but the experimenter considered that they were not fairly conducted, as the fish had been injured in the transportation. It is obvious that in every trial of this nature great attention to this part of the operation must be requisite.

Since this, a perfect experiment to this effect has been made by Colonel Meynell, in Yorkshire. The fish have lived three years, and it is understood that they have propagated abundantly. They were not affected by freezing, as the whole pond, which contained

about three acres, was so frozen over as to admit of skating. As to their quality, it was considered by the fishermen of the Tees, by whom the pond was drawn, that they had never seen "a finer lot of Smelts;" so that in this case there was no loss of flavour or quality.

The common pike, Esox Lucius, which is an inhabitant of fresh lakes with us, is also found in the Caspian sea; proving that this animal among others is indifferent to the quality of the water which it inhabits, and, in this case, permanently so.

It seems to be unquestionable, that in the Dee and some other Scottish rivers, the common eel, Muræna Anguilla, migrates annually to the sea, wherever it has the power of reaching it; returning again to the rivers and lakes which it has generally been supposed permanently to inhabit. The conger eel, Muræna Conger, which is an inhabitant of the sea, in general, also frequents rivers; so that, of this genus, there are two at least of which the residence is occasionally convertible.

The Gadus Callarias, or torsk, is also known to enter the mouths of rivers, so that it can reside at least for a time in fresh waters without injury; but it is not known to remain in them permanently. That the Gadus Morhua, or common cod, can reside permanently in fresh water, is proved in Shetland. In the mainland, as it is termed, of that group, the inlet called Stromness-voe communicates with an inland fresh-water lake by a channel so narrow as to admit of a rude bridge by which the opposite shores are connected. In this fresh water, cod are frequently taken; and that the water is perfectly fresh is certain; as the tide is never sufficient to pass the strait of communication, merely damming the fresh water till the ebb again commences. The inhabitants seem to entertain no doubt that the cod remains there for a considerable time; but the subject not having particularly interested them, it remains to be discovered whether their residence is permanent or occasional, or whether they spawn there. If they reside there, even for any length of time, it is probable that this water contains other sea-fishes, by which they are tempted, unless they feed on trout; but I could not discover that any others had been found.

The Gadus Barbatus, or whiting, and the Tricirratus, or rockling, occur in abundance in those Highland sea-lochs where the water is at times perfectly fresh, from the magnitude of the rivers in rainy seasons; not quitting their haunts even when it is deeply tinged by the colour derived from peat. From their permanence in those situations, and from being taken of all sizes, they probably spawn there; and, if so, they offer, like the common river flounder and the pike, perfect examples of the permanent convertibility of the habits of these species.

The Cottus Quadricornis, a native of the Baltic, also ascends rivers, as does the GasterosteusPungitius, or stickleback, in our own country. The Pleuronectes Platessa, or plaice, as has already been observed, is naturalized to fresh water in East Friesland: and the P. Flessus, or common flounder, is well known to be permanent in the Thames and other rivers, far within the fresh water, although equally a salt-water fish. The P. Roseus has also been taken in the Thames. I am further informed that a sole was kept in a fresh-water pond in a garden, by a person whose name I need not quote, for a great many years; and if the plaice and flounder can be so naturalized, it is not unlikely that this would prove true of the whole genus.

Although the mackerel is rare in Scotland, it is sometimes taken in the lochs of the western highlands, where the water, from the entrance of rivers, is nearly or absolutely fresh; a proof at least, as in many other fishes, that whatever aversion they may have to residing permanently in fresh water, whether from the want of food or for other reasons unknown to us, they experience no difficulty in respiring in it.

The Mugil Cephalus, or mullet, which is a sea-fish, not only ascends rivers, but has been introduced and detained in ponds; offering another example, like the plaice, of the possibility of permanently naturalizing a sea-fish to fresh water. This fish does not necessarily spawn in rivers; since, in England, it performs this operation on the sandy and muddy shores of the sea. Yet, in Asia minor, it appears that it always spawns in the rivers, ascending the Sturmus, the Meander, and others for this purpose, and pro-

ducing the Botargo so well known in the market. This is a valuable fact in the question under consideration; as it proves that, in the matter of spawning, fishes are not tied down to those fixed and necessary habits which has been commonly supposed.

As the case of the naturalization of the grey mullet is particularly interesting, and is at the same time unknown, except to the few individuals who caused the experiment to be made at random, it deserves a more particular description; since it offers, together with the instance of the cod in Shetland, another of those facts which have come within my own knowledge.

This experiment is, at the same time, perfect, as much so as that of the plaice in East Friesland; and it holds out therefore a tempting prospect of success in other cases where no trials, either from accident or design, have yet been made.

About ten years ago a number of the grey mullet, about the size of the finger, were placed in a pond of three acres in area, in Guernsey; the water being perfectly fresh. They have since increased in size, as well as in numbers; although, from the small extent of this pond, it is evident that their ultimate increase cannot be very considerable. Fish of four pounds in weight have since been taken from this pond, so that in this respect as well as in their propagation, the experiment is complete and perfectly satisfactory. It is remarked that they are much fatter than those taken in sea-water in their natural state, but that the flavour is not so good.

From this pond a number of small fish were afterwards taken for the purpose of stocking a smaller one. These continued to grow and thrive for about three years; when unfortunately, the occurrence of a severe frost, during which the water was closed up many days, destroyed them.

In this case it is evident that nothing is wanting to the establishment of the fact in question with regard to the grey mullet: and it may safely therefore be named as one of the fish which may without difficulty be naturalized to fresh water, and made use of to increase the accessible variety of our food or luxury, in places where fresh waters abound, and which are far removed from the sea.

This experiment is fully confirmed by the practice of the

Sicilians. In the Lake Biviere, this fish is cultivated for the purpose of food, and because its quality is thus found to be improved, it is an important circumstance also, that the water is here such as would be supposed peculiarly offensive to fishes taken from the sea; as it lies in a marshy plain, and is such that the extent of the lake is twice as great in winter as in summer. Such water must be nearly putrid; and therefore the Mullet at least would probably live and thrive in any ditch or pond.

As its quality is thus also found to be improved, it is plain that the report respecting the deterioration of the Guernsey Mullets is, at best, doubtful; while it is equally probable, from this case, as well as that of Colonel Meynell's Smelts, that the general effect would be to improve, instead of injuring, the flavour of the sea-fishes in general.

Though here somewhat out of place, I may also notice, that Lobsters and Crabs are introduced into the same lake for similar purposes, where they are not only preserved but improve in flavour. It had been concluded, in England, that these animals could not be so cultivated, because an experiment made by Sir Charles Monck failed. We must probably attribute this to some accident; as the Sicilian practice is of long standing, and has been confirmed through an unknown course of years. As to the improvement of the flavour of the Lobsters and Crabs in this case, it is distinctly stated, and it confirms the general presumption that this would commonly be the result; while another confirmation is found in the fact that Oysters acquire their good qualities only by residence in fresh water. Thus the Oysters of Portsmouth and elsewhere are transferred to Colchester; and if those which are called "Natives," possess good qualities, it is because they are produced at the æstuaries of rivers, where the water is considerably fresh, as is the case with those of Milton. In a similar way, Cockles and Muscles are perfectly worthless, except in analogous situations, as is equally the case with Periwinkles; and it is known to every one, that the best Shrimps are those which are taken on the fresh and muddy shores of England.

The Clupea Sprattus, or sprat, is well known to be taken in the fresh

water of the Thames; although it is not ascertained whether it remains for any length of time out of the salt or brackish water. The C. Alosa, however, or shad, ascends rivers to spawn in the spring, like the salmon, returning in the autumn; and its spawn, the white-bait of London epicures, is well known to be taken in the fresh water. It is probable therefore that it spawns there, as the salmon does; and hence also, were this fish worth the experiment, it might probably be naturalized to lakes and ponds. This seems peculiarly plausible in the case of all the sea-fish which spawn in fresh waters; because this is one of the natural operations which we should conceive it *à priori*, most difficult to counteract.

I already noticed that the best known fish of this genus, the herring, was found in the fresh American rivers. And though I was obliged to contradict M. Nouel respecting its existence in Loch Lomond, I may here say that it has been found at different times in Loch Dhu, a fresh water lake in Argyllshire, near Loch Fyne. In this case, it appears to have been introduced during a flood, through the small river by which this piece of water communicates with the sea; being afterwards confined by the subsidence of the water, so as to have remained imprisoned for many years. It does not seem however to have been ascertained whether or not it propagated in the lake; so that this natural experiment still remains incomplete for want of observation. This however is a trial so easily repeated, that nothing probably has prevented it, but that ignorance or prejudice on this subject which it is the main object of this paper to remove, by holding out reasons for probable success.

The Crucian, Cyprinus Carassius, the Bleak, C. Alburnus, the roach, C. Rutilus, the bream, C. Brama, the C. Idus, C. Nasus, C. Aspius, and C. Ballerus, like the pike, seem to inhabit the Caspian sea as well as the fresh waters and ponds of Europe; offering other instances of perfect and permanent indifference to the nature of the waters in which they exist.

The Chalcoides, in the same genus, migrates annually from the same salt lake to the rivers that run into it; and the C. Aphya

seems to inhabit indifferently the sea shores and the mouths of the neighbouring rivers.

The Cyclopterus Liparis has also been observed to ascend from the sea into fresh waters; and the same fact is familiar with respect to the sturgeon, the common lamprey, and the lesser lamprey, or Petromyzon Fluviatilis.

Lastly, the Delphinus Leucas, or white whale, is known to ascend the fresh-water rivers of Northern America; but as this animal breathes air, it does not, in that point at least, coincide with the true fishes, which respire water. The appearance of this species of whale seems to have been the chief evidence by which Hearne and Mackenzie attempted to prove that they had reached the sea in their respective expeditions. It is known to ascend the Hudson to a distance of 100 miles and more, above the salt water, and is taken by an established fishery high up in some of the fresh rivers of Hudson's-bay.

Here then is a large body of evidence, derived not only from the occasional, but from the permanent, residence of many sea-fish in fresh waters, and, on the contrary, of some fresh-water fish in salt lakes, to prove the existence, or possibility, of these convertible habits, at least in those species. But it will be convenient to subdivide the considerations which arise out of this subject, as they affect those functions in fish which, as far as this question is concerned, must be considered as of a vital or essential nature; either as they regard the life and health of the individual, or the continuation of the species.

The first of these is the act of respiration. The first doubt naturally arising on this subject is, whether salt-water fish can with impunity breathe fresh water, and the contrary. From the great number of the sea-fish which, either systematically or occasionally, visit fresh water without inconvenience, it is fair to conclude that the latter in no way disagrees with the function of respiration in them. A much stronger confirmation of this is afforded by the facility with which the plaice, mullet, and flounder have been permanently naturalized to fresh water; and by the fact that so many

others which are described in the preceding catalogue, seem by nature to inhabit both indifferently. It remains indeed to be proved that any fresh-water species now known as limited to rivers and lakes, can be permanently confined to the sea; but this is a point which can obviously never be determined.

A species of argument might be derived, on this subject, from the probable state of the earth at former distant periods, and from that which has probably been the original condition of many inland lakes besides the Caspian. It is probable that many such lakes were portions of the salt ocean, and that they have been rendered fresh since their separation from it, by the effects of the rivers flowing into them. In this case, the fish which these contain were once sea-fish; and thus perhaps we may account for the double existence of the pike and of those Cyprini above described, in the salt waters of the Caspian and in the fresh lakes of other inland districts. But I will not here lay much stress on this reasoning. It is evident at least, from the preceding remarks, that a change of the medium of respiration is not injurious or poisonous to all those fish which even incidentally pass into fresh waters from the sea, as this effect, if any, ought to be immediate, or at least speedy. If so many species can bear that change in the medium of respiration, it is not unlikely that the whole might, as the general structure of the respiratory organ is the same in all; and it is not therefore likely that this function will be the cause of any great obstruction in attempts to change permanently the residence of fishes from one variety of water to another.

The next important function to be considered is that of nutrition, or the probability that food may be found or provided for those sea-fish which any projects of naturalizing them in fresh waters, may confine to inland lakes. We are so little acquainted with the food of many fishes, that it is not possible to throw much light on this subject; but it is probable that the most important and insurmountable obstacle will be found here. Of many species, it seems to be ascertained that they feed on marine vegetables. Others, like the mullet, are known to plough the sand in search of lumbrici; probably also, of the spawn of other fishes. Some species

seem to be especially provided with the means and the desire of feeding on shell-fish; others on crabs or the crustaceous insects; while the northern whale, by an arrangement which must always appear extraordinary, is furnished only with the power of subsisting on animals so small as to be imperceptible, to its sense of sight at least, and which, in the scale of dimensions, lie almost at the opposite extreme to its enormous bulk. Many fish, like the cod, are known to be omnivorous; and of others, it appears probable that they feed solely on the multitudinous tribes of vermes and insects which crowd the waters. It is probable that, with respect to a great number of species, they live in succession on each other, if that expression can be used with propriety; or that, in the myriads of animals of singular and imperfect construction, and often of microscopic minuteness, which crowd the ocean to a degree that almost surpasses credibility, provision is made for the wants, in succession, of all those which successively exceed each other in size, voracity, or activity.

If we were to judge from what is within our reach with respect to many fishes, we should be tempted to imagine that they can live for long periods, even without food, or with a very small proportion. Thus the cod, one of the most voracious, has been kept in perfect condition in Orkney, confined in sea-ponds for three months and more; although no visible animal was admitted with the water which the tide daily brought to its prison. During the whole residence of the salmon in fresh waters, which often extends to a considerable period, it seems to exist with little food; since the few winged insects at which it occasionally rises, can afford no effectual nutrition to an animal of such bulk and activity. The state of the common ornamental gold-fish confined in water-glasses, is equally remarkable; but it is unnecessary to prolong the enumeration of facts which, however difficult to explain, have ong been familiar to those conversant with the habits of fishes.

But whatever we may doubt respecting the nature or the necessary quantity of food for fishes, it must be evident that no permanent naturalization of many of them, at least, can be expected, unless the new situation is such as to provide them with a suffi-

cient supply of food. In many cases, perhaps, we may judge for them; and if the proprietor of a Highland lake chooses to eat cod rather than pike, at the expense of a proportion of his perch and trout, and can persuade them to live in his fresh water, it is probable that they will not have to lament the want of food.

In any case, our ignorance on this subject need not be a bar in the way of any experiment on this kind of naturalization. So many species find their food without our knowing the means or the materials, that we may safely trust to their wants and their powers. Besides, as the enormous reproduction of all these tribes is evidently in part destined for the general support in succession of all those of which they are the prey, it is evident that by increasing the population and the variety in any of these watery kingdoms, we increase the means of mutual support. The smaller feed on that which the larger could not find or use; and thus they maintain the existence of their superiors, who, in return, are destined perhaps to maintain them with their own ova or offspring. If again, practically, the plaice and the flounder, natives of the sea, have found the means of permanently feeding themselves in fresh waters, it is not unlikely that many others may there find food unknown to us, and, for want of trial, unknown at present even to them.

But there is no difficulty in feeding them, should that prove necessary. This was a common practice with the Romans; and those who choose to turn to Varro or Columella, may see records of the immense sums which were expended by the Romans in feeding the fish in their vivaria; as they may also see, from the enormous prices paid by Cæsar, Lucullus, and others, to what an extent the practice of keeping fish-ponds was carried, and how important a branch of rural economy this was considered. The consequence attached to fishes by this people is apparent everywhere; and no one need be told of the celebrated Senatusconsultum held on a turbot, or of the fishes which, Martial tells us, came to their owner's call and licked his hands. If, in our own rural economy, it is found profitable to feed pigs and fowls, it would not be less so to feed fish, nor are these tribes, apparently, less omnivorous than hogs.

The last of the important functions of fishes likely to be an impediment to this attempt, is their reproduction, or the act of spawning; or rather, the circumstances necessary to ensure the vivification of the ova. The instincts, as they are called, or the peculiar habits of many fish in this important affair, seem often to be as obstinate as they are peculiar. This is notorious in the case of the salmon; which must not only deposit the ova in a river, but in a remote part of it, and even in the very stream in which it has itself been produced. Many fishes deposit their eggs only on shallow shores, although they inhabit the deep seas. Some frequent the estuaries of rivers for that purpose, others select mud, a third set sand, and others again the crevices of rocks. Yet as this part of the economy of fishes is a matter of necessity, it only remains to consider whether, being deprived of these conveniences to which they are instinctively addicted, they would not soon find it expedient to abandon them, and to adopt those alone which were within their reach. In this respect, the habits of the land animals with which we are acquainted, have been found susceptible of temporary, and even of permanent changes. Little acquainted as we are with the intellectual powers of fishes, or with the variety of character and capacity for education which may exist among them, it is bad reasoning to presume that they are incapable of cultivation or change of habits, and that their sole talents are to catch, and their sole occupation to eat, each other.

Presuming, therefore that the ova must, as a matter of necessity, be deposited somewhere, it may be observed that inland lakes present all the varieties of bottom which are found in the sea. They receive rivers, have muddy bottoms, sandy and gravelly shores, and intricate rocky creeks; and, in some or other of these places, every fish may find a situation for its ova, more or less consonant to its natural habits. Nor is there any reason to suppose that where the parent lives, its offspring could not be vivified; since the vitality of the ova is far less likely to be affected by a change from salt water to fresh, than the complicated functions of the living and full grown animal. In a practical view, the power of continuing the species under such a change, is proved by the facts already cited with re-

spect to the plaice, mullet, and flounder; and it is only to be regretted that no further evidence of this satisfactory nature can be adduced in favour of this reasoning. The double residence, however, of the pike, and of the various Cyprini, already more than once quoted, offers a complete argument in favour of the convertible habits of these species at least, in the business of reproduction as in that of food.

Supposing now that, at least the probability of all these reasonings is admitted, it only remains to put these speculations to the test of more extensive experiments. Nature has executed two, perhaps more; art, in the plaice, the smelt, and the mullet, has carried three more into effect. There appears no practical difficulty attending it; as fish can be transported alive in water, for a great length of time, and to great distances, without inconvenience. If Shetland were differently constituted with respect to the distribution of its population and the residence of its proprietors, a very satisfactory and easy experiment, on the cod at least, might be made in Stromness Voe. It would only be necessary to shut up the very narrow opening by which it communicates with the fresh water, by means of a grating, and time alone would soon determine the question. Should this paper meet the eyes of a body of proprietors distinguished for their intelligence and activity, it may perhaps induce him in whose power it lies, to make this easy experiment. Nor could there, in this place, as in some other situations in Scotland, be any difficulty in extending the same trials to other species of fish. But I need not dwell on this part of a subject which every one is competent to understand, but which not many have the means of submitting to the test of experiment.

On the transportation of fish, I must remark that it is not attended with so much difficulty as is commonly imagined, and that the fault generally has lain with those who have made the attempts. Many fish are exceedingly tolerant of being out of water for a time. The carp is kept in nets, in cellars, and fed thus in Holland. Minnows will live for months, crowded in a quart pot, with as little water as they can barely stir in, or in absolute contact. The whole of the flat fish are similarly tenacious of life; as are the conger, the gur-

nard tribe, the dog-fishes, and many more which I need not enumerate. The fault of those who have attempted the transportation, has been to take fishes which had been long hooked, dragging upon Long Lines, or entangled for a night or more in a trammel net. Owing to the peculiar distribution of the arteries in fishes, their muscular power is speedily exhausted by violent exertion; and hence they are literally killed, or nearly so, before they are taken out of the water in such cases; an effect which, in the case of salmon and trout taken by a fly, is vulgarly called drowning. This must be avoided; and it is well known that when cod are taken by hand lines, and thence transferred to the wells of the fishing boats, they always live, unless the gills or stomach have been much injured by the hook.

As far as this may be considered a question of economy or utility, it is not necessary to say much. It may perhaps, abstractedly, be deemed of little consequence whether an inhabitant of Germany is condemned to eat roach and gudgeon, or to regale on whiting and smelts; or whether, in a Highland lake, john-dory is to be substituted for pike, and turbot for par. But all the improvements in the details of human life may, if we please, be measured by the same rule. We have naturalized and domesticated the wild animals that walk and fly, to be our fellow-labourers, our companions, our servants in the chase, our amusement, and our food. Nature has given us crabs and sloes, which we have converted by our industry and perseverance into golden pippins and green gages. It is not an illaudable pursuit to apply to the uses of man all those bounties which nature has spread around him; but on the possession and perfect enjoyment of which this law has been stamped, that without labour and industry, they shall not be attained.

Yet while on this question of economy, it may not be improper to suggest a few doubts respecting the prudence of that conduct which, in this country, neglects the sources of rural profit to be derived from cultivating the produce of its fresh waters. In France, it is said that the value of an acre of water is equal to that of an acre of land; and these ponds are rented by great fishermen, or fishmongers, who adapt these systems of fishing their farms in

such a manner as to ensure the greatest possible permanent stock of fish; removing the superfluous produce, which would otherwise be devoured or die, without injuring the future population, and thus procuring a constant and regular supply in the season, without the risk of exhaustion.

In Germany, it is well known that the cultivation of carp and other fresh-water fish is a regular object of attention; and although the proximity of the sea may cause us to treat with contempt the painful efforts of our neighbours to do that for themselves which nature has so bountifully done for us, it is assuredly not unworthy the attention of the proprietors of inland counties in Britain, to attempt to produce from them, either rent or profit. Under the present system, the fresh waters of this country are of little use but to furnish amusement to the sectaries of good Isaac Walton, and occupation to those who create flies of which no entomologist ever dreamed. Amusement would not be excluded by profit. If, too, it is said, as it well may be, that, as an article of food, the fresh-water fish are inferior to those of the sea, it must also be remembered that variety, no less than excellence, is one of the great resources, as it is one of the main pursuits, of the noble science of gastronomy.

But, to be more serious, the quantity of fresh waters existing in Britain is so considerable, as, with the exception of Switzerland, to exceed those of any country in Europe. From these, no profit whatever is derived. A Scottish lake, under a regular system of fishing and care, might probably far exceed in value the miserable tract of bog and rock by which it is enclosed. The canals of this country occupy a respectable space, and might, like ponds, be stored with fish, to the probable advantage of the proprietors no less than of the community. Even the rivers are unproductive, with the solitary exception of salmon, and of eels; since the quantity of other fresh-water fish brought to market is far too insignificant to be an object of attention in a case like this where so much more might be effected.

The objection to fishing on canals is the injury which may be done to the banks. That, if it really exists, would cease whenever

the fishery should become a farm in the hands of a lessee. In all these cases it is merely supposed that, as in France and Germany, the object should be the cultivation of fresh-water fish. But if as the views held out in this paper attempt to prove, sea fish can be naturalized in canals, lakes, ponds, and rivers, it is not unlikely that the sources of profit might be materially increased. Experience would in a certain time teach us to know the fish that would live together most usefully for ourselves, that would rather contribute to each other's support and to ours, than to their own mutual extermination. As yet, this is a subject little known, because it has been too much the usage to suppose, that as man cannot live in the same element with a fish, he has no chance of acquiring a knowledge of its habits and pursuits.

The lakes of Scotland, of the North of England, and of Wales, offer particular facilities for the naturalization of sea fish, on account of the small distance at which most of them lie from the sea, and of the consequent facility of transporting these creatures in a living state. Should such a project ever be carried into effect, the good consequences are obvious. The facility of commanding a supply of fish would be increased; while that would also become certain, since it would no longer depend on weather, which so often interferes with the regularity of the sea fishery and of the market. The demand and supply might then also be more accurately balanced, as it is in all parallel cases when the steady price of domestic animals for food, is compared with that of those which are the produce only of chance or contingency. It is an unquestionable fact that the produce of fish for consumption may be much increased by the very act of fishing them; or that a certain proportion may be regularly taken away for use, without diminishing this subaqueous population. It is thus that a profit is made by waters which in their natural state yield no supply for man. Nor, in the sea, is the apparent supply for our uses, ever diminished by any quantity which we can consume, provided that, in some peculiar cases, care is taken not to destroy the ova, or the fish under a certain size. How little attention has been paid to this subject, in sea fishing, is proved by a recent Act of Parlia-

ment regulating the use of trawl nets in Torbay, and by other regulations of less value, which have occasionally been made for similar purposes.

In the cultivation of fish in fresh waters. the whole management becomes so completely under our command, that there would be no difficulty in framing such regulations as increase of knowledge would soon suggest, and as private interest would follow, or that of the public enforce.

In what precise manner the regularity of fishing increases the supply, or at least does not diminish the production, has not been clearly ascertained. That the several species eat each other's ova and young, and even their own, is very well established. Many devour each other, even at full growth, and it is not unlikely that many also die of disease or want of food. In such cases the steady removal of the superfluous part of the population cannot check its increase. If all the Turks and Egyptians who are to die of the plague next year, were to be devoured by crocodiles, there would be a certain quantity of food gained, and every thing would go on just as before. The empire would not have been a bit less populous or prosperous if the Huns and the Ostrogoths had eaten each other instead of strewing their own bones and those of their antagonists on the banks of the Danube, or the plains of the Campagna.

Respecting the species which might probably succeed in fresh water, it is not possible to offer any very rational conjectures. It is probable that they might most effectually be sought among those genera of which some species are already known to be versatile in their habits. In those genera of animals at least which are natural and not artificial, there are considerable resemblances among the habits and pursuits of the different species. Thus it is not very improbable that as the plaice, the flounder and the mullet, have been naturalized to fresh water, the whole of the fishes of analogous habits, and particularly those of the genus Pleuronectes, might be habituated to inland lakes. The turbot and the sole would be very desirable objects of cultivation. If different species of Gadus have been shown to be at least indifferent to the quality of

the water into which they enter, the whiting as well as the cod might possibly learn to inhabit our lakes or rivers, and thus become among the most accessible as it is among the most delicate of fishes. If the smelt could be naturalized in ponds, as I have here rendered more than probable, it would, from the esteem in which it is held, be a peculiarly desirable acquisition. The hints contained in this paper may possibly induce others, who have the means in their power, to assist in the execution of a set of trials which can succeed only in the hands of many, and which must necessarily be the work of time.

It has been suggested that as the flavour of fresh-water fish is far inferior to that of the marine species, the effect of naturalization would be to diminish their value as articles of food. This does not absolutely follow, although it may be thought probable from the case of the mullet above-mentioned, and by the fact that the flavour of the salmon is constantly diminishing from the time it has quitted the sea. If such should prove to be the case, it might indeed diminish the value of the acquisition, but it would not therefore destroy it; nor is it likely that a smelt would ever sink to the scale of a gudgeon, or a whiting to that of a roach.

I have already shewn, however, that this deterioration of quality, so far from being probable, is not at all likely to occur; since with this single exception, supposed to have occurred in Guernsey, and which is probably the report of prejudice, the flavour is really improved in all the cases where the experiment has been fairly tried; and since the transportation, in Sicily, is made with this very object and no other. At any rate, let the trials be made before any such condemnatory judgment is passed.

I will only further remark here, that there is no very good reason why the turtle should not be naturalized. What an acquisition this would be, it may be left to the Court of Aldermen to decide. The animals of hot climates, that live in air, have been so; and and why the submarine, or amphibious ones should not equally admit of this change of habits, I know not, and nobody else does. The turtle might take its place alongside of the peacock and the pintado, and with his fellow turtles of the land; while, if he chose

to hybernate, he might find a dormitory in Loch Lomond or elsewhere, to pass the chilling hours of a Highland winter. And the change would be less than in the case of the land animals; since there is not such a difference of temperature in the one case as in the other.

While on this subject, it will not be out of place to mention a parallel object of economy, far less known than it merits, and indeed little known out of Scotland, where it has been practised, although in a very limited manner, for many years. This is the preservation of sea fish in salt-water ponds. There are three of these in Scotland; one in Galloway, another in Fife, and the third in Orkney. In these, even cod are known to live for many months, and to increase in size, without any loss of quality, and without any other food than that, imperceptible to us, which is brought by the daily influx of the sea. In the pond in Galloway, some individual cod have been living for many years, so as to have become tame, if such a word may be applied to a fish, feeding, like hogs, out of a trough when introduced with a supply of food.

This practice is so obvious an extension, as it is an improvement, of the expedient of using well-boats, as to afford cause of surprise that it has not been adopted by those who are interested. Motives of interest in the proprietors would shortly become matter of advantage to the consumers; and the unsteadiness of a fish-market, no unimportant object of municipal attention, even in London, would cease to be a subject of complaint,

The Romans, who seem to have far exceeded us in all that relates to eating, as they did in a few other matters, were well acquainted with this practice; and the history of their Vivaria has descended to us, with much more that relates to their rural economy, of which this formed a distinguished branch. Columella says, decidedly, that they transported the spawn of various sea fishes to the different fresh-water lakes round Rome, "marinis seminibus implebant," and that this was a regular trade with the early agriculturists of the rustic Republic, before the rich and luxurious took the keeping of artificial Vivaria into their own hands. He mentions the Mugil, which is probably our mul-

let, together with " lupos, auratasque," two fishes of which we are not now able with certainty to assign the names. He farther alludes to others which he has not named, as being " dulcis aquæ tolerantia." He then passes from the subject, as of too familiar a nature to require a more detailed notice; a stronger proof than even his enumeration would have been, of the facts which I have thus attempted to support from his authority, and of the established existence of a practice which we have lost, and appear, very strangely, to be unwilling to revive. But I must refer your readers to the original, for the whole of this curious chapter, as the translation of it would inconveniently prolong this paper.

The merely temporary naturalization to our lakes and ponds in the case of sea fish, would be no light acquisition to the gastronomer who might desire to have turbot before the season, or to reserve it at five shillings for consumption, when the price has risen to three guineas. If the cod chooses to live in the fresh lake of Stromness-voe, there is no reason why we should not keep them in our own gardens till the day of giving a dinner comes round, or why Mr. Groves should not render the Serpentine a park for surmullets, instead of allowing it to be consigned to frogs and tadpoles. It is to be hoped that the Fishmongers' Company will take these matters to heart, as in duty bound; and that, in the progress of perfectibility, even the odious canal in St. James's Park may become a repository of turtles, instead of what it now is, a Stygian nursery of Malaria and his black host.

There is a subsidiary question arising out of these speculations respecting the convertibility of the habits of marine animals, highly interesting to geology, and on which it will not be out of place to say a few words, although unfortunately not much solid information can be procured respecting it. This relates to the power which many, perhaps all of the vermes inhabiting shells, possess of residing indifferently in fresh or in salt water.

It is well known to geologists that with respect to many, if not all of those deposits supposed to have been formed, like that of Paris and of England, under fresh water, the question mainly rests

on this, namely, whether the shells now supposed, from certain analogies and peculiarities of structure, to have been inhabitants of fresh-water lakes, may not have equally existed in salt lakes, or even in the sea. Some experiments towards the elucidation of this subject have been instituted in France; but I need not detail them, as they must be fresh in the recollection of all the readers of this Journal. It has also been recently ascertained by M. Freminville, that in the gulf of Livonia, the shell fish which usually inhabit the sea, and those which belong to fresh waters, are found living together in the same places. While these confirm the general presumption which forms the basis of this communication, their general probability is also strengthened by that analogy. A few facts of common occurrence on our own shores, seem to add additional weight to the opinion that the testaceous fishes in general are not rigidly limited to one kind of water, but are capable of living in both.

On our sea coasts, the common muscle is invariably larger and fatter at the entrance of fresh-water streams into the sea, particularly if these bring down mud, and in these places the water is scarcely salt; yet they live also and propagate in abundance on shores which receive no fresh water. The oyster is transported from the sea to brackish water, where it also, not only lives, but improves in condition. In the same manner the common cockle inhabits indifferently the muddy sand-banks near the æstuaries of rivers, which are always soaked with fresh water, and those sandy or half muddy shores where no such water is found. These are by no means the whole of the instances which might be enumerated in support of an opinion, of which the determination is so important in the present state of geological science; but as this subject is too important to pass lightly over, and as the bounds of this communication are already exceeded, I shall leave it to those who may have the means and the inclination to examine it in greater detail. I will only add, that the same considerations will lead to similar doubts, where it has been attempted by geologists to determine the nature of strata, as to their marine or fresh water origin, by that of the remains of fishes found in them.

Art. V. *Description of* Mr. Cooper's *Lamp Furnace, for the Analysis of Organic Bodies.*

Having had occasion to use Mr. Cooper's lamp for the analysis of organic bodies, described in the last volume of the Transactions of the Society of Arts, and having found it very effectual, we have taken the following account of it from that work, with an abstract of the method of using it; and are enabled by Mr. Cooper's kindness to add the description of some improvements which he has since made on the original apparatus.

Fig. 1. Plate iv. *a a* and *b b*, are two long spirit-lamps, each having ten burners and wicks, the burners of each lamp sloping towards those of the other, as seen in the end view, fig. 2. They are placed in a tin tray *c c*, mounted on four feet. This tray is perforated in the middle the whole length of the lamp, and as wide as *e e*, fig. 2. The object in sloping the burners is, that they may clear the lamps and approach each other as near as is requisite, yet leave free space for a current of air, the tray being perforated and mounted on feet for this purpose: *d d* are spring wires at each end of the tray, to receive the tube *f f* containing the substance to be analyzed, and to hold it over the flames; by pressing the shoulders *g g*, fig. 2, the wires open to receive the tube, and close on removing the pressure; and should the tube be shorter than the lamps, an additional support on a leaden foot, fig. 3, is placed through the opening *e e* of the tray to rise between the flames, and hold the end of the tube.

The tubes are coated with copper foil, wrapped spirally round them; if each succeeding fold be on half the other, there will be a double coat of copper all the way, if on two-thirds, there will be three layers of copper, by which the glass tube is prevented from bending when hot, and becomes very uniformly heated. The spirals are continued beyond the end of the tube to reach the support, and leave the end within the flames. The dotted line at *h*, fig. 4, shews the end of the tube short of the support, the foil is secured at the last coil by binding wire, as at *i*.

Fig. 5, shews the foil in act of being wrapped on, also the proportion of the space occupied by the materials; first the mixture of oxide of copper with the material to be analyzed, next pure oxide of copper, or copper filings, and lastly asbestos. When the quantity of water formed is considerable, the tube is either blown into a bulb, as at *k*, fig. 6, or melted on to one ready prepared.

Fig. 7, is a long funnel, made by drawing out the end of a tube of suitable thickness at *m*, till it is long and small enough through *n n* to reach to the bottom of the tube, and then cutting it off at *m*, by which liquids may be introduced to the bottom of the tube without soiling the sides.

As the wicks nearest the trough are to be first lighted, and the remainder in succession as the former finish their action, there are upright supports of tin *o o* fixed on the lamps, one for each space between the burners, against which to rest a slip of tin *p p*, to prevent the lighted wicks from kindling those next, and it also enables the experimenter to extinguish those which have done duty. In fig. 2, the tin slip *p p* is shewn by dotted lines reaching from lamp to lamp. Little flat caps are put on each burner when done with, to prevent waste of spirit; fig. 8 shews one of these caps *q* in its place. *r r*, fig. 1, is a shelf fixed to the mercurial trough, to hold the lamps; *s s*, the graduated jar. The pipes, with corks, *w w*, fig. 2, are the apertures by which the spirit is poured into the lamps; their places only are marked at *w w*, fig. 1. The whole of this apparatus is made of tin plate.

At first Mr. Cooper operated with a tube of one piece; and the materials being put in when the tube was straight, it was afterward heated and bent at the open extremity, so as to suit the mercurial trough; but this has been improved upon by making the tube shorter and having a bent piece, attached to it by a small flexible tube of caoutchouc, *f*, fig. 1. It removes the chance of accident from stiffness in the end of the tube, and the tubes being straight, may be used many times in succession.

Mr. Cooper has also used with advantage, at times, the form of

receiver shewn at fig. 9; it is about twelve inches long, and one inch in diameter, and being filled with mercury and hung over a basin is ready for use. When containing gas, its quantity is estimated by the graduated scale on the tube, care being taken previously to compensate for any difference of mercurial pressure by inserting the long funnel and cork, fig. 10, into the mouth of the receiver, and pouring mercury into the funnel until it is level with that in the receiver. It is easy afterwards to admit water or solution of potash into the receiver to absorb the carbonic acid, and leave the nitrogen.

The oxide of copper required in using this instrument may be procured either by burning the residuum of verdigris which has been used in the preparation of acetic acid, or by heating plates of copper with access of air, and quenching them in water. Great care should be taken that the oxide be pure, and it should be pulverised and heated in a crucible, stirring at the same time. It may then be sifted, and the different portions preserved apart. The tube used should be of crown or green-bottle glass, fourteen to fifteen inches long, (not so long if the separate bent end piece is used,) and from one to two tenths of an inch internal diameter; it should be clean and dry, one end should be sealed up by a blow-pipe, and then it may be balanced. The substance if volatile is now to be introduced, if solid it may be shaken to the bottom, if fluid it is to be poured in by the funnel, fig. 7. The quantity of substance is then to be ascertained, and a portion of cold oxide of copper introduced, sufficient to absorb the substance if fluid, and cover it about a quarter of an inch; after which recently heated and still warm oxide is to be added to the proper height. Then a portion of recently ignited asbestos is introduced and pressed rather lightly on to the oxide, and occupying from one to two inches. The tube, with its contents, is then to be balanced again, after which it is to be enveloped in the copper foil, (care being taken that the foil does not cover the part containing the asbestos,) and the end piece with its caoutchouc tube is to be fastened on.

The tube is then to be arranged as in the figure, and heat applied; the lamps are to have but short wicks, so that the top of the flame

shall just touch the tube, and only one set will be required, unless the tube be large, as for instance, half an inch in diameter *; the lamps are to be lighted in succession, those nearest the gazometer first.

If the substance to be analyzed be a vegetable salt, or be hygrometric, it must be dried, which is best done in vacuo, but which Mr. Cooper effects also in the following manner. A wide-mouth stoppered bottle is selected, and also a smaller bottle which will easily go into it; a quantity of dry pulverized chloride of calcium is then strewed over the bottom of the larger bottle, and the smaller, containing the substance to be dried is also introduced; a small piece of bibulous paper is moistened with alcohol and put inside the larger phial; it is then lighted, and when it has burned for a second or two, the stopper is put into the bottle, and the vacuum obtained is such that the desiccation goes on very rapidly and effectually.

When substances of this kind are analyzed, they must, of course, be mixed with oxide of copper before they are introduced into the tube; a quantity of pure oxide is then to be put into the tube, and it is as well to add afterwards a small quantity of copper filings or shavings. In heating the tube the wicks are to be lighted as before, but instead of suffering the whole to burn at once, it is as well to leave only three or four in action at a time, extinguishing the others, but taking care to ignite the whole extent of tube at once at the end of the process.

When nitrogen is present in the body to be analyzed it has a tendency to become oxidized at high temperatures by the oxide of copper, and in this case yields erroneous results. To obviate this as much as possible Mr. Cooper has lately used protoxide of copper, instead of peroxide; and though he finds that in certain circumstances this also will impart oxygen to the nitrogen, yet it does so with far greater difficulty than the peroxide: hence in all cases where nitrogen is concerned, the protoxide should be used. The protoxide is prepared by fusing peroxide of copper with copper filings in excess; a mass of protoxide is obtained, which, on being pulverized and sifted is fit for use.

* The power of the lamps is such that a thick platinum tube, half an inch in diameter, may be rendered bright red-hot by them.

Art. VI. *Description of a self-acting Blowpipe.* By Mr. H. B. Leeson.

It has, I believe, before been observed that bottles of Indian Rubber might be expanded to a considerable size by condensing air into them: I am not, however, aware that bottles so filled with condensed air have been applied to the purposes of a Blowpipe.

The bottles I make use of vary in weight from half to three-quarters of a pound, and may be readily procured at the Stationer's. To prepare them they should be boiled in water till completely softened, which, if they are put into water already boiling, will generally be accomplished in ten minutes or a quarter of an hour. They must then be taken out and suffered to cool, when a brass tube may be fitted into the neck of the bottle, having a small cock screwed into it at one end, by which it may be connected with the condensing syringe, and to which the blowpipe jets may be attached. There should be a milled projection on the side of the tube, for the purpose of more firmly attaching the bottle to it, which may be effected by passing a ligature of waxed string round the neck of the bottle on each side of the above-mentioned projection.

The bottle must next be filled with condensed air. After a few strokes of the syringe a blister will be observed to form, which will gradually enlarge till the greatest part of the bottle (which must be selected uniform in substance, and free from defects.) has extended to a similar substance. The condensation should not then be continued farther.

Bottles of the size I have mentioned will generally extend from fourteen to seventeen inches in diameter without bursting; and I have occasionally extended them much beyond these dimensions; but in this the operator must, of course, be entirely directed by his own observations.

The Indian rubber varies in its quality. There is one sort which appears of a blacker hue before extension, but becomes very thin and almost transparent on condensing air into it, whilst there is

another sort having a browner colour, which is much less yielding in its substance, and cannot be extended to the same thinness as the former.

I have found both sorts to answer my purpose, but the above observations may be useful in determining the quantity of air which may be condensed into the bottles with safety.

To apply these bottles when filled with condensed air, nothing more is necessary than to remove the syringe, and in its place to screw on a jet of such bore as may be required. On opening the cock the air will be expelled by the elasticity of the India Rubber, and its own condensation, in a strong and uniform stream, which in bottles of the size I have mentioned will continue from twenty-five minutes to an hour, according to the size of the jet.

When once prepared the bottles may be constantly expanded to the same dimensions without any danger of bursting. When the air is exhausted, the bottles will be found somewhat enlarged in dimensions, but may again be contracted by holding them before a fire, or a few minutes' immersion in boiling water. This, however, is unnecessary, since no subsequent inflation will be found to increase the size of the bottle any further, and I have used the same repeatedly without any apparent diminution of its elastic powers.

The principal advantages of this blowpipe are its great portability, and length and steadiness of action, (in which I consider it much superior to the hydraulic blowpipe,) together with the perfect liberty at which, when properly mounted, it leaves the operator's hands.

This blowpipe is applicable to any of the gases, and may, I conceive, be applied with advantage to contain the explosive mixture of oxygen and hydrogen, as no inconvenience can possibly accrue from its bursting, beyond the loss of the bottle.

This blowpipe may be supplied with air or gas during an experiment, by having a separate communication for the syringe into the piece of tube before mentioned, and this will enable the operator to continue his experiments for any period of time.

Blowpipes on this construction may be procured, very neatly and conveniently mounted, at Mr. Newman's, No. 8, Lisle-Street, Leicester-Square.

ART. VII. ASTRONOMICAL PHENOMENA arranged in Order of Succession, for the Months of July, August, and September in the Year 1824. By JAMES SOUTH, F.R.S.

(Continued from Page 84.)

JULY.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
1	Sun		6 41	23 7 N
	Im. *	7.8	15 19	or 8^h 39′ MT.
	*'s R.A. 11^h 2′ Decl. 0° 26′ S. (cont.)			
	Georgian .		19 1	23 9 S
	Mercury..		5 15	20 42 N
	Venus ...		6 12	23 35 N
2	Sun		6 45	23 3 N
	Moon....		11 48	5 7 S
	Georgian .		19 1	23 9 S
	Mercury..		5 20	21 0 N
	Venus ...		6 17	23 34 N
3	Sun		6 50	22 59 N
	Moon....		12 41	10 57 S
	Georgian .		19 1	23 9 S
	Mercury..		5 26	21 17 N
	Venus ...		6 23	23 32 N
4	Sun		6 54	22 53 N
	Moon....		13 35	16 6 S
	Georgian .		19 1	23 9 S
	Mercury..		5 31	21 35 N
	Venus ...		6 28	23 31 N
5	Sun		6 58	22 48 N
	Moon....		14 30	20 20 S
	Georgian .		19 1	23 9 S
	Mercury..		5 38	21 51 N
	Venus ...		6 34	23 30 N
6	Sun		7 2	22 42 N
	Moon....		15 26	23 26 S
	42 Libræ.	5.6	15 30	23 14 S
	XV. 192..	6	15 43	23 27 S
	XV. 225..	3	15 50	22 6 S
	Georgian .		19 0	23 10 S
	Im. *	6	19 15	or 12^h 15′ MT.
	*'s R.A. 15^h 34′ Decl. 23° 50′ S. (3′S.)			
	Em.		20 16	or 13^h 16′ MT.
	Mercury..		5 45	22 6 N
	Venus ...		6 39	23 29 N
7	Sun		7 6	22 36 N
	Moon....		16 24	25 14 S
	25 Scorpii	6	16 36	25 12 S
	18 Oph...	6	16 39	24 19 S
	26 —— ...	6	16 45	21 43 S
	Georgian .		19 0	23 10 S
	Mercury..		5 52	22 22 N
	Venus ...		6 45	23 27 N
8	Sun		7 10	22 29 N
	42 Oph...	3.4	17 11	24 49 S
8	44 Oph...	5.6	17 16	24 0 S
	Moon....		17 21	25 42 S
	XVII. 142	8	17 25	24 30 S
	Georgian .		18 59	23 10 S
	Mercury..		5 59	22 34 N
	Venus ...		6 50	23 21 N
9	Sun		7 14	22 22 N
	Moon....		18 17	21 47 S
	24 Sagit..	7	18 23	24 14 S
	XVIII. 129	6	18 28	23 39 S
	—— 141	6	18 31	23 59 S
	Georgian .		18 59	23 10 S
	Im. *	7.8	21 56	or 14^h 44′ MT.
	*'s R.A. 18^h 24′ Decl. 24° 21′ S. (12′N.)			
	Em.		22 50	or 15^h 38′ (7′N.)
	Mercury..		6 7	22 47 N
	Venus ...		6 56	23 16 N
10	Sun		7 18	22 14 N
	Im. * 1 ..	8	15 49	or 8^h 34′ MT.
	*'s R.A. 19^h 6′ Decl. 22° 48′ S. (10′N.)			
	Em. * 1..		16 43	or 9^h 28′ MT.
	Im. * 2 ..	6	17 52	or 10^h 37′ MT.
	*'s R.A. 19^h 10′ Decl. 22° 43′ S. (5′N.)			
	o Sagit ...	1.5	18 51	22 0 S
	π Sagit...	4.5	18 59	21 18 S
	Georgian .		18 59	23 10 S
	Em. * 2..		19 5	or 11^h 50′ MT. (1′S.)
	Moon....		19 10	22 40 S
	Im. * 3 ..	8	19 41	or 12^h 26′ MT.
	*'s R.A. 19^h 12′ Decl. 22° 24′ S. (cont.)			
	Im. * 4 ..	6.7	22 0	or 14^h 44′ MT.
	*'s R.A. 19^h 16′ Decl. 22° 7′ S. (14′N.)			
	Em. * 4..		22 33	or 15^h 17′ MT. (11′N)
	Im. * 5 ..	8	22 42	or 15^h 26′ MT.
	*'s R.A. 19^h 17′ Decl. 21° 58′ S. (cont.)			
	Eclipse of Moon } --		22 46	or 15^h 30′ MT.
	Moon sets eclipsed.			
	Mercury..		6 14	22 59 N
	Venus ...		7 1	23 10 N
11	Sun		7 22	22 7 N
	Im. * 1 ..	8	15 11	or 7^h 52′ MT.
	*'s R.A. 19^h 56′ Decl. 19° 59′ S. (8′N.)			
	Em. * 1..		16 16	or 8^h 57′ MT. (0′)
	Im. * 2 ..	8	16 59	or 9^h 40′ MT.
	*'s R.A. 19^h 59′ Decl. 19° 55′ S. (1′N.)			
	Im. * 3 ..	8	17 43	or 10^h 24′ MT.

JULY.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.
11	*'s R.A. 20^h 0' Decl. 19° 53' S. (4'S.)			
	Im. * 4 ..	8	17 44 or 10^h 25' MT.	
	*'s R.A. 20^h 0' Decl. 19° 34' S. (12'N.)			
	Im. * 5 ..	8	17 45 or 10^h 26' MT.	
	*'s R.A. 20^h 0' Decl. 19° 53' S. (3'S.)			
	Em. * 4 ..		18 42 or 11^h 23' MT. (5'N)	
	Em. * 3 ..		18 45 or 11^h 26' MT. (11'S)	
	Em. * 5 ..		18 47 or 11^h 28' MT. (11'S)	
	Georgian .		18 59	23 11 S
	Moon....		20 3	19 32 S
	XX. 80 ..	7.8	20 11	18 52 S
	π Capr...	5	20 17	18 47 S
	XX. 151..	6	20 20	19 9 S
	Im. * 6 ..	8	21 22 or 14^h 2' MT.	
	*'s R.A. 20^h 6' Decl. 19° 38' S. (5'S.)			
	Em. * 6 ..		22 13 or 14^h 53' MT. (13'S)	
	Mercury..		6 22	23 5 N
	Venus ...		7 6	23 4 N
12	Sun		7 27	21 58 N
	Georgian .		18 59	23 11 S
	XX. 341 .	7.8	20 43	13 51 S
	Moon....		20 52	15 36 S
	XXI. 7...	7.8	21 2	15 11 S
	29 Capr..	5	21 6	15 54 S
	Mercury..		6 30	23 12 N
	Venus ...		7 12	22 59 N
13	Sun		7 31	21 50 N
	Georgian .		18 59	23 11 S
	17 Aquar.	6	21 14	10 4 S
	XXI. 134.	7.8	21 19	12 20 S
	ξ Aqu....	5	21 28	8 38 S
	Moon....		21 38	11 4 S
	Mercury..		6 38	23 18 N
	Venus ...		7 17	22 53 N
14	Sun		7 35	21 41 N
	Georgian .		18 59	23 12 S
	Moon....		22 23	6 10 S
	Mercury..		6 47	23 18 N
	Venus ...		7 [illegible]	22 44 N
15	Sun		7 39	21 32 N
	Georgian .		18 58	23 12 S
	Mercury..		6 57	23 17 N
	Venus ...		7 28	22 31 N
16	Sun		7 44	21 22 N
	Georgian .		18 58	23 12 S
	Mercury..		7 6	23 17 N
	Venus ...		7 33	22 25 N
17	Sun		7 47	21 12 N
	Georgian .		18 58	23 12 S
	Im. *....	7	20 6 or 12^h 23' MT.	
	*'s R.A. 3^h 44' Decl. 22° 41' N. (10' N.)			
	Em.....		21 12 or 13^h 29' MT. (4'S.)	
17	Mercury..		7 5	23 9 N
	Venus ...		7 38	22 15 N
18	Sun		7 51	21 1 N
	Georgian .		18 58	23 13 S
	Mercury..		7 24	23 1 N
	Venus ...		7 44	22 6 N
19	Sun		7 55	20 51 N
	Georgian .		18 58	23 13 S
	Mercury..		7 33	22 53 N
	Venus ...		7 49	21 56 N
20	Sun		7 59	20 39 N
	Georgian .		18 58	23 13 S
	Mercury..		7 42	22 37 N
	Venus ...		7 54	21 43 N
21	Sun		8 3	20 28 N
	Georgian .		18 57	23 14 S
	Im. * 1 ..	7.8	22 4 or 14^h 5' MT.	
	*'s R.A. 4^h 2' Decl. 23° 7' N. (2'N.)			
	Em. * 1 ..		22 55 or 14^h 56' MT. (6'S.)	
	Im. * 2 ..	7	23 0 or 15^h 1' MT.	
	*'s R.A. 4^h 4' Decl. 23° 8' N. (5'S.)			
	Em. * 2 ..		23 45 or 15^h 46' MT. (11'S)	
	Mercury..		7 51	22 21 N
	Venus ...		7 59	21 29 N
22	Sun		8 7	20 16 N
	Georgian .		18 57	23 14 S
	Im. *....	6	20 55 or 12^h 52' MT.	
	*'s R.A. 4^h 57' Decl. 24° 1' N. (4'N.)			
	Em.....		21 5 or 13^h 2' MT. (2'N.)	
	Mercury..		8 0	22 5 N
	Venus ...		8 4	21 16 N
23	Sun		8 11	20 4 N
	Georgian .		18 57	23 14 S
	Im. * 1 ..	8	23 17 or 15^h 10' MT.	
	*'s R.A. 6^h 8' Decl. 23° 40' N. (11 N.)			
	Im. * 2 ..	7.8	23 24 or 15^h 17' MT.	
	*'s R.A. 6^h 8' Decl. 23° 40' N. (11'N.)			
	Im. * 3 ..	7	23 28 or 15^h 21' MT.	
	*'s R.A. 6^h 9' Decl. 23° 32' N. (3'N.)			
	Im. * 4 ..	8	23 33 or 15^h 26' MT.	
	*'s R.A. 6^h 9' Decl. 23° 20' N. (9'S.)			
	Im. * 5 ..	8	23 45 or 15^h 38' N	
	*'s R.A. 6^h 9' Decl. 23° 32' N. (3'N.)			
	Em. * 1 ..		23 51 or 15^h 44' MT. (11'N)	
	Em. * 2 ..		23 58 or 15^h 51' MT. (11'N)	
	Em. * 4 ..		0 10 or 16^h 3' MT. (9'S)	
	Em. * 3 ..		0 13 or 16^h 6' MT. (3'N)	
	Em. * 5 ..		0 31 or 16^h 24' MT. (3'N)	
	Mercury..		8 9	21 44 N
	Venus ...		8 10	21 2 N
24	Sun		8 15	19 52 N
	Venus ...		8 15	20 49 N

JULY.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
24	Georgian .		18 57	23 14 S
25	Sun		8 19	19 39 N
	Mercury. .		8 19	21 22 N
	Venus ...		8 20	20 35 N
	Georgian .		18 57	23 15 S
26	Sun		8 23	19 26 N
	Venus ...		8 25	20 18 N
	Mercury. .		8 27	20 54 N
	Georgian .		18 57	23 15 S
27	Sun		8 27	19 12 N
	Venus ...		8 30	20 2 N
	Mercury. .		8 35	20 25 N
	Georgian .		18 57	23 15 S
28	Sun		8 31	18 58 N

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
28	Mercury. .		8 44	19 57 N
	Georgian .		18 57	23 15 S
29	Sun		8 34	18 44 S
	Venus ...		8 35	19 45 N
	Mercury. .		8 52	19 24 N
	Georgian .		18 57	23 15 S
30	Sun		8 38	18 30 N
	Venus ...		8 41	19 28 N
	Mercury. .		9 0	18 51 N
	Georgian .		18 57	23 15 S
31	Sun		8 42	18 15 N
	Venus ...		8 46	19 12 N
	Mercury. .		9 8	18 17 N
	Georgian .		18 57	23 15 S

AUGUST.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
1	Sun		8 46	18 0 N
	Venus ...		8 51	18 55 N
	Mercury. .		9 16	17 41 N
	Moon....		14 12	19 2 S
2	Sun		8 50	17 45 N
	Venus ...		8 56	18 35 N
	Mercury. .		9 24	17 3 N
	Moon....		15 9	22 32 S
3	Sun		8 51	17 29 N
	Venus ...		9 1	18 14 N
	Mercury. .		9 32	16 25 N
	Moon....		16 6	24 50 S
	Im. *	4	18 32 or $9^h 43'$ MT.	
	*'s R.A. $16^h 10'$ Decl. $25° 10'$ S. (14′S.)			
	Em.		19 5 or $10^h 15'$	(13′S.)
4	Sun		8 58	17 18 N
	Venus ...		9 6	17 54 N
	Mercury. .		9 40	15 46 N
	Moon....		17 3	25 37 S
	θ Oph....	3.4	17 11	24 49 S
	44 —	5.6	17 16	24 0 S
	XVII. 142		17 25	24 30 S
5	Sun		9 2	16 57 N
	Venus ...		9 10	17 38 N
	Mercury. .		9 47	15 5 N
	63 Oph...	6.7	17 44	24 51 S
	5 Sagit...	7	17 49	24 16 S
	XVII. 342	7	17 51	24 24 S
	Moon....		17 59	25 9 S
6	Sun		9 5	16 41 N
	Venus ...		9 15	17 13 N
	Mercury. .		9 54	14 24 N

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
6	Im. * 1 ..	8.9	16 27 or $7^h 26'$ MT.	
	*'s R.A. $18^h 51'$ Decl. $23° 28'$ S. (11′N.)			
	Em. * 1..		17 25 or $8^h 24'$ MT.	(7′N.)
	28 Sagit. .	6	18 36	22 31 S
	30 ——. .	6	18 40	22 21 S
	35 ——. .	5	18 45	22 53 S
	Moon....		18 51	23 26 S
	Im. of Georgian		19 4 or $10^h 3'$ MT.	
	Im. * 2 ..	8	19 54 or $10^h 53'$ MT.	
	*'s R.A. $18^h 55'$ Decl. $23° 6'$ S. (cont.)			
	Em. Georgian		20 16 or $11^h 15'$ MT.	
7	Sun		9 9	16 21 N
	Venus ...		9 20	16 52 N
	Mercury. .		10 1	13 42 N
	XIX. 176.	7	19 26	19 14 S
	56 Sagit. .	6	19 36	20 10 S
	57 ——. .	5.6	19 42	19 29 S
	Moon....		19 46	20 39 S
	Im. *	8	22 6 or 13^h [illegible] MT.	
	*'s R.A. $19^h 50'$ Decl. $20° 20'$ S. (14′N)			
	Em.		23 7 or $14^h 0'$ MT.	(0′)
8	Sun		9 13	16 7 N
	Venus ...		9 25	16 29 N
	Mercury. .		10 8	13 0 N
	13 Capr. .	6	20 27	15 45 S
	XX. 240. .	6.7	20 31	16 45 S
	Moon....		20 35	17 0 S
	XX. 367. .	8	20 45	15 57 S
9	Sun		9 17	15 50 N
	Venus ...		9 30	16 6 N
	Mercury. .		10 15	12 18 N
	Im. *	9	19 8 or $9^h 55'$ MT.	

AUGUST.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
9	*'s R.A. 21^h 20′ Decl. 12° 50′ S. (14′N.)			
	Em.		20 1 or 10^h 47′ MT. (6′N.)	
	XXI. 82	7.8	21 12	12 12 S
	XXI. 134	7.8	21 19	12 19 S
	Moon		21 23	12 41 S
	λ Capric.	5.6	21 37	12 10 S
10	Sun		9 21	15 32 N
	Venus		9 35	15 13 N
	Mercury		10 21	11 35 N
	30 Aquar.	5.6	21 54	7 22 S
	XXI. 403	8	21 59	7 14 S
	XXII. 14	8	22 3	7 20 S
	Moon		22 8	7 55 S
	Im. * 1	8.9	0 49 or 15^h 31′ MT.	
	*'s R.A. 22^h 12′ Decl. 7° 8′ S. (13′N.)			
	Im. * 2	7	0 50 or 15^h 32′ MT.	
	*'s R.A. 22^h 12′ Decl. 7° 8′ S. (12′N.)			
	Im. * 3	8.9	1 1 or 15^h 43′ MT.	
	*'s R.A. 22^h 13′ Decl. 7° 4′ S. (12′N.)			
	Em. * 1		1 54 or 16^h 36′ MT. (0′)	
	Em. * 2		1 56 or 16^h 38′ MT. (2′S.)	
	Em. * 3		2 11 or 16^h 53′ MT. (2′S.)	
11	Sun		9 24	15 15 N
	Venus		9 40	15 20 N
	Mercury		10 27	10 52 N
	Im. *	8.9	20 37 or 11^h 16′ MT.	
	*'s R.A. 22^h 51′ Decl. 3° 23′ S. (6′S.)			
	Em.		21 23 or 12^h 2′ MT. (15′S.)	
	Moon		22 53	2 52 S
	XXIII. 68	6.7	23 15	0 40 S
	12 Pisc.	7	23 20	2 0 S
	11 ——	6.7	23 25	2 13 S
12	Sun		9 28	14 57 N
	Venus		9 45	14 57 N
	Mercury		10 31	10 8 N
	Im. * 1	6	16 30 or 7^h 6′ MT.	
	*'s R.A. 23^h 27′ Decl. 1° 8′ N. (1′N.)			
	Em. * 1		17 [illegible] or 8^h 2′ MT. (7′S.)	
	γ Piscium	4.5	23 8	2 20 N
	7 ——	6	23 11	4 25 N
	17 ——	4.5	23 31	4 41 N
	Im. * 2		23 36 or 14^h 10′ MT.	
	*'s R.A. 23^h 37′ Decl. 2° 31′ N. (13′S.)			
	Moon		23 38	2 17 N
	Em. * 2		0 40 or 15^h 14′ MT. (1′S.)	
13	Sun		9 32	14 38 N
	Venus		9 50	14 34 N
	Mercury		10 40	9 25 N
	Im. *	6	20 8 or 10^h 39′ MT.	
	*'s R.A. 0^h 17′ Decl. 6° 43′ N. (13′N.)			
	Em. *		20 53 or 11^h 24′ MT. (3′N.)	
	Moon		0 22	7 22 N
14	Sun		9 36	14 20 N
	Venus		9 55	14 9 N
	Mercury		10 46	8 42 N
	Im. *	7.8	0 36 or 15^h 2′ MT.	
	*'s R.A. 1^h 9′ Decl. 12° 42′ N. (5′N.)			
	Em. *		1 54 or 16^h 20′ MT. (9′S.)	
15	Sun		9 40	14 1 N
	Venus		10 0	13 43 N
	Mercury		10 51	7 58 N
16	Sun		9 43	13 42 N
	Venus		10 5	13 18 N
	Mercury		10 57	7 15 N
	Im. * 2	7	22 28 or 12^h 47′ MT.	
	*'s R.A. 2^h 45′ Decl. 19° 39′ N. (8′N.)			
	Im. * 1	7	22 50 or 13^h 9′ MT.	
	*'s R.A. 2^h 44′ Decl. 19° 51′ N. (cont.)			
	Em. * 2		23 29 or 13^h 47′ (2′S.)	
	Im. * 3	6	0 17 or 14^h 35′ MT.	
	*'s R.A. 2^h 48′ Decl. 19° 57′ N. (9′S.)			
	Em. * 3		1 21 or 15^h 39′ MT. (2′S)	
17	Sun		9 47	13 23 N
	Venus		10 9	12 53 N
	Mercury		11 3	6 31 N
	Im. *	7.8	0 1 or 14^h 16′ MT.	
	*'s R.A. 3^h 42′ Decl. 22° 9′ N. (cont.)			
	Im. * 2	7	1 9 or 15^h 23′ MT.	
	*'s R.A. 3^h 44′ Decl. 22° 41′ N. (6′N.)			
	Em. * 2		2 15 or 16^h 29′ MT. (1′S.)	
	Im. * 3	6.7	2 25 or 16^h 39′ MT.	
	*'s R.A. 3^h 47′ Decl. 22° 39′ N. (4′S.)			
	Em. * 3		3 29 or 17^h 43′ (10′S.)	
18	Sun		9 51	13 4
	Venus		10 14	12 27 N
	Mercury		11 9	5 47 N
	Im. * 1	7.8	20 36 or 10^h 48′ MT.	
	*'s R.A. 4^h 31′ Decl. 23° 41′ N. (9′N.)			
	Im. * 2	7	21 4 or 11^h 15′ MT.	
	*'s R.A. 4^h 33′ Decl. 23° 45′ N. (10′N.)			
	Em. * 1		21 16 or 11^h 27′ MT. (5′N.)	
	Em. * 2		21 41 or 11^h 55′ (5′N.)	
19	Sun		9 54	12 44 N
	Venus		10 19	12 2 N
	Mercury		11 15	5 4 N
20	Sun		9 58	12 25 N
	Venus		10 24	11 34 N
	Mercury		11 20	4 22 N
21	Sun		10 2	12 5 N
	Venus		10 28	11 6 N
	Mercury		11 26	3 40 N
22	Sun		10 6	11 44 N
	Venus		10 33	10 39 N
	Mercury		11 31	2 57 N

AUGUST.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
23	Sun		10 9	11 24 N
	Venus....		10 38	10 11 N
	Mercury..		11 36	2 15 N
24	Sun		10 13	11 4 N
	Venus....		10 42	9 44 N
	Mercury..		11 40	1 33 N
25	Sun		10 17	10 43 N
	Venus ...		10 47	9 17 N
	Mercury..		11 45	0 52 N
26	Sun		10 20	10 22 N
	Venus . .		10 51	8 48 N
	Mercury..		11 50	0 12 N
27	Sun		10 24	10 1 N
	Venus....		10 56	8 19 N
	Mercury..		11 55	0 27 S
28	Sun		10 28	9 40 N

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
28	Venus ...		11 0	7 50 N
	Mercury..		12 0	1 7 S
29	Sun		10 31	9 19 N
	Venus ...		11 5	7 22 N
	Mercury..		12 4	1 45 S
	Moon....		14 48	21 12 S
30	Sun		10 35	8 57 N
	Venus ...		11 9	6 53 N
	Mercury..		12 9	2 23 S
	Moon....		15 46	23 58 S
	Im. *....	7	19 20 or 8^h 44′ MT.	
	*'s R.A. 15^h 52′ Decl. 24° 31′ S. (cont.)			
31	Sun		10 39	8 35 N
	Venus....		11 14	6 24 N
	Mercury..		12 13	3 1 S
	Moon....		16 45	25 19 S

SEPTEMBER.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
1	Sun		10 42	8 14 N
	Venus ...		11 19	5 55 N
	Mercury..		12 16	3 38 S
	Moon....		17 41	25 16 S
2	Sun		10 46	7 52 N
	Venus ...		11 23	5 25 N
	Mercury..		12 20	4 13 S
	Moon....		18 36	23 58 S
	ν 1 Sagit..	5	18 41	22 57 S
	XVIII. 255	6.7	18 51	22 56 S
	——— 294	6.7	18 56	22 45 S
3	Sun		10 49	7 30 N
	Venus ...		11 28	4 55 N
	Mercury..		12 24	4 18 S
	Im. * 1 ..	7.8	19 11 or 8^h 19′ MT.	
	*'s R.A. 19^h 20′ Decl. 21° 42′ S. (7′S.)			
	Im. * 2 ..	8	19 16 or 8^h 24′ MT.	
	*'s R.A. 19^h 30′ Decl. 21° 39′ S. (4′S.)			
	Moon....		19 29	21 30 S
	56 Sagit..	6	19 36	20 10 S
	57 ——..	5.6	19 42	19 29 S
	XIX. 377.	8	19 55	21 48 S
	Em. * 1..		20 3 or 9^h 11′ MT. (13′S.)	
	Em. * 2..		20 18 or 9^h 26′ MT. (12′S.)	
4	Sun		10 53	7 8 N
	Venus ...		11 32	4 25 N
	Mercury..		12 28	5 23 S
	Im. * 1 ..	5	19 2 or 8^h 7′ MT.	
	*'s R.A. 20^h 19′ Decl. 18° 23′ S. (3′S.)			
	Im. * 2 ..	7.8	19 13 or 8^h 18′ MT.	

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
4	*'s R.A. 20^h 19′ Decl. 18° 27′ S. (7′S.)			
	Im. * 3 ..	6.7	19 50 or 8^h 55′ MT.	
	*'s R.A. 20^h 19′ Decl. 18^h 1′ S. (15′N.)			
	Em. * 2..		20 6 or 9^h 10′ MT. (15′S.)	
	XX. 45 ..	8	20 6	16 49 S
	Em. * 1..		20 13 or 9^h 17′ MT. (12′S.)	
	Em. * 2..		20 15 or 9^h 19′ MT. (10′N)	
	Moon....		20 20	18 11 S
	XX. 194..	7	20 26	17 7 S
	XX. 240..	6.7	20 31	16 45 S
5	Sun		10 57	6 45 N
	Venus ...		11 37	3 54 N
	Mercury..		12 32	5 55 S
	Im. * 1 ..	7	20 40 or 9^h 41′ MT.	
	*'s R.A. 21^h [illegible] Decl. 14° 0′ S. (9′N.)			
	Im. * 2 ..	[illegible]	20 42 or 9^h 43′ MT.	
	*'s R.A. 21^h 6′ Decl. 13° 55′ S (cont.)			
	8 Aquar..	6	20 50	13 41 S
	ν ———..		21 0	12 5 S
	Moon....		21 7	14 7 S
	18 Aquar.		21 15	13 38 S
	Em. * 1..		21 58 or 10^h 59′ MT. (4′S.)	
	Im. * 3 ..	7	22 42 or 11^h 42′ MT.	
	*'s R.A. 21^h 10′ Decl. 13° 43′ S. (4′N.)			
	Em. * 3..		23 55 or 12^h 55′ MT. (9′S.)	
6	Sun		11 0	6 28 N
	Venus ...		11 41	3 24 N
	Mercury..		12 35	6 26 S
	Im. * 1 ..	7.8	17 35 or 6^h 32′ MT.	
	*'s R.A. 21^h 48′ Decl. 10° 24′ S. (7′S.)			

SEPTEMBER.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars.	Sidereal Time.	Planet's or Star's Declination.
			H. M.	D. M.
6	Em. * 1..		18 14 or 7h 11' MT. (14'S.)	
	ξ Aquar..	5	21 28	8 38 S
	46 Capr..	6	21 36	9 53 S
	Im. * 2 ..	7.8	21 38 or 10h 34' MT.	
	*'s R.A. 21h 54' Decl. 9° 21' S. (12'N.)			
	Moon....		21 53	9 30 S
	XXII. 44.	4.5	22 8	8 30 S
	Em. * 2..		22 51 or 11h 47' MT. (8'S.)	
7	Sun		11 4	6 1 N
	Venus ...		11 46	2 54 N
	Mercury..		12 39	6 58 S
	60 Aquar.	6.7	22 25	2 28 S
	XXII. 183	7.8	22 32	4 28 S
	Moon....		22 39	4 31 S
	XXIII. 17	7.8	23 5	3 35 S
8	Sun		11 7	5 38 N
	Venus ...		11 51	2 23 N
	Mercury..		12 42	7 26 S
	Im. * 1 ..	6	19 39 or 8h 27' MT.	
	*'s R.A. 23h 18' Decl. 0° 10' N. (5'N.)			
	Em. * 1..		20 32 or 9h 20' MT. (3'N.)	
	XXIII. 15	8	23 5	1 15 N
	γ Piscium	4.5	23 8	2 20 N
	κ ———	5.6	23 18	0 18 N
	Moon....		23 23	0 33 N
	Im. * 2 ..	9	0 41 or 13h 28' MT.	
	*'s R.A. 23h 25' Decl. 1° 2' N. (5'N.)			
	Em. * 2..		1 55 or 14h 42' MT. (11'S.)	
	Im. * 3 ..	6	2 23 or 15h 10' MT.	
	*'s R.A. 23h 27' Decl. 1° 8' N. (11'S.)			
	Em. * 3..		2 32 or 15h 19' MT. (13'S.)	
9	Sun		11 11	5 15 N
	Venus ...		11 55	1 52 N
	Mercury..		12 45	7 54 S
	26 Pisc...	6	23 46	6 6 N
	ω ——...	4.5	23 50	5 44 N
	Moon....		0 8	5 52 N
	45 Pisc...	6	0 17	6 43 N
10	Sun		11 [illegible]	4 53 N
	Venus ...		12 0	1 21 N
	Mercury..		12 48	8 22 S
	O 149 ...	7.8	0 32	12 0 N
	58 Pisc...	6	0 38	11 1 N
	O 247 ...	8	0 49	11 11 N
	Moon....		0 55	10 34 N
11	Sun		11 18	4 30 N
	Venus ...		12 5	0 50 N
	Mercury..		12 50	8 45 N
	Im. *	6.7	17 39 or 6h 16' MT.	
	*'s R.A. 1h 30' Decl. 13° 23' N. (4'S.)			
	Em.		18 29 or 7h 6' MT. (16'S.)	
	Moon....		1 44	15 3 N
12	Sun		11 22	4 7 N
	Venus ...		12 9	0 20 N
	Mercury..		12 53	9 7 S
	Im. *	6.7	20 17 or 8h 50' MT.	
	*'s R.A. 2h 21' Decl. 18° 6' N. (15'N.)			
	Em.		20 55 or 9h 28' MT. (8'N.)	
13	Sun		11 25	3 44 N
	Venus ...		12 11	0 11 S
	Mercury..		12 55	9 30 S
	Im. 1 Sat..		2 31 or 14h 59' MT. (100+)	
	Im. *	6.7	2 56 or 15h 24' MT.	
	*'s R.A. 3h 28' Decl. 22° 5' N. (15'N.)			
	Em.		3 32 or 16h 0' MT. (12'N)	
14	Sun		11 29	3 21 N
	Venus ...		12 18	0 42 S
	Mercury..		12 57	9 47 S
	Im. *	7	0 11 or 12h 35' MT.	
	*'s R.A. 4h 20' Decl. 23° 11' N. (9'S.)			
	Em.		0 54 or 13h 18' MT. (13'S.)	
15	Sun		11 33	2 58 N
	Venus ...		12 23	1 42 S
	Mercury..		12 59	10 4 S
	Im. * 1 ..	6	3 11 or 15h 31' MT.	
	*'s R.A. 5h 25' Decl. 23° 55' N. (5'S.)			
	Im. * 2 ..	8	3 24 or 15h 44' MT.	
	*'s R.A. 5h 24' Decl. 24° 10' N. (10'S.)			
	Em. * 1..		4 12 or 16h 32' MT. (6'S.)	
	Em. * 2..		4 16 or 16h 36' MT. (9'S.)	
16	Sun		11 36	2 35 N
	Venus ...		12 27	1 43 S
	Mercury..		13 0	10 21 S
17	Sun		11 40	2 11 N
	Venus ...		12 31	2 14 S
	Mercury..		13 1	10 30 S
	Im. *	7.8	0 12 or 12h 24' MT.	
	*'s R.A. 7h 17' Decl. 20° 36' N. (5'S.)			
	Em.		1 2 or 13h 14' MT. (2'S.)	
18	Sun		11 43	1 48 N
	Venus ...		12 36	2 41 S
	Mercury..		13 [illegible]	10 38 S
19	Sun		11 47	1 25 N
	Venus ...		12 40	3 15 S
	Mercury..		13 2	10 47 S
20	Sun		11 51	1 1 N
	Venus ...		12 45	3 45 S
	Mercury..		13 2	10 47 S
	Im. 1 Sat..		4 53 or 16h 53' MT. (100+)	
21	Sun		11 54	0 38 N
	Venus ...		12 49	4 16 S
	Mercury..		13 2	10 46 S
22	Sun		11 58	0 15 N
	Venus ...		12 54	4 46 S

SEPTEMBER.

Days.	Planet's or Star's Name, &c.	Magnitude of Stars	Sidereal Time. H. M.	Planet's or Star's Declination. D. M.
22	Mercury..		13 2	10 45 S
23	Sun		12 1	0 9 S
	Venus ...		12 59	5 17 S
	Mercury..		13 0	10 33 S
24	Sun		12 5	0 32 S
	Mercury..		12 59	10 20 S
	Venus ...		13 3	5 47 S
25	Sun		12 9	0 56 S
	Mercury..		12 57	10 7 S
	Venus ...		13 8	6 18 S
26	Sun		12 12	1 19 S
	Mercury..		12 54	9 41 S
	Venus ...		13 12	6 47 S
27	Sun		12 16	1 43 S
	Mercury..		12 51	9 15 S
	Venus ...		13 17	7 17 S
	Moon....		16 20	24 41 S
28	Sun		12 19	2 6 S
	Mercury..		12 48	8 49 S
	Venus ...		13 21	7 46 S
	Moon....		17 19	25 12 S
29	Sun		12 23	2 29 S
	Mercury..		12 45	8 13 S
	Venus ...		13 26	8 16 S
	Moon....		18 17	24 20 S
	Im. * 1 ..	9	19 33 or 6h 59' MT.	
	*'s R.A. 18h 20' Decl. 24° 10' S. (8'N.)			
	Em. * 1..		20 40 or 8h 6' MT.(2'N.)	
	Im. * 2 ..	7	21 10 or 8h 36' MT.	
	*'s R.A. 18h 22' Decl. 24° 14' S. (5'S.)			
	Im. * 3 ..	6.7	21 32 or 8h 58' MT.	
	*'s R.A. 18h 23' Decl. 24° 9' S. (3'S.)			
29	Im. * 4 ..	8	21 36 or 9h 2' MT.	
	*'s R.A. 18h 23' Decl. 24° 15' S. (9'S.)			
	Im. * 5 ..	6.7	21 54 or 9h 19' MT.	
	*'s R.A. 18h 24' Decl. 24° 9' S. (5'S.)			
	Em. * 2..		22 4 or 9h 29' MT.(11'N)	
	Em. * 4..		22 17 or 9h 42' MT.(14'S.)	
	Em. * 3..		22 31 or 9h 56' MT.(10'S.)	
	Em. * 5..		22 46 or 10h 11' MT.(11'S.)	
30	Sun		12 27	2 53 S
	Mercury..		12 42	7 37 S
	Venus ...		13 30	8 45 S
	Im. * 1 ..	7.8	18 42 or 6h 4' MT.	
	*'s R.A. 19h 10' Decl. 22° 7' S. (15'N.)			
	Im. * 2 ..	7	19 2 or 6h 24' MT.	
	*'s R.A. 19h 12' Decl. 22° 24' S. (5'S.)			
	Em. * 1..		19 3 or 6h 25' MT.(13'N)	
	Moon....		19 11	22 18 S
	50 Sagit..	6.7	19 16	22 7 S
	XIX. 138.	6	19 20	21 40 S
	—— 166.	7	19 25	21 9 S
	Em. * 2..	7	20 4 or 7h 26' MT.(12'S.)	
	Im. * 3 ..	5	20 14 or 7h 36' MT.	
	*'s R.A. 19h 13' Decl. 22° 5' S. (6'N.)			
	Em. * 3..		21 27 or 8h 49' MT.(4'S.)	
	Im. * 4 ..	8	21 31 or 8h 56' MT.	
	*'s R.A. 19h 16' Decl. 21° 58' S. (7'N.)			
	Im. * 5 ..	6.7	21 49 or 9h 11' MT.	
	*'s R.A. 19h 16' Decl. 22° 7' S. (9'S.)			
	Em. * 5..		22 27 or 9h 49' MT.(14'S.)	
	Im. * 6 ..	8	22 34 or 9h 56' MT.	
	*'s R.A. 19h 16' Decl. 21° 35' S. (cont.)			
	Em. * 4..		22 44 or 10h 6' MT.(2'S.)	

Art. VIII. *On the Soundings in the British Channel.*

[To the Editor of the *Quarterly Journal.*]

Sir.—A paper has been lately read at the Royal Irish Academy by Mr. A. Nimmo, civil engineer, containing the ingenious idea that the various coloured sands, shells, and ooze found at the bottom of the sea, in the chaps of the Channel, are the terminations of beds of granite, limestone, coal, *&c.*, which dip from various parts of Great Britain and Ireland, and to which they may be satisfactorily traced.

This idea, if properly pursued, would materially assist in classifying the soundings in our Channel charts. It is needless to insist upon the great importance of a correct projection of those soundings; in conjunction with the depth of the water, they are frequently the only means that the seaman possesses, in thick weather and long nights, of ascertaining his position; and it is too well known that the most part of the soundings in both the channels, and the whole of them on the great western bank, are laid down on our present charts in a manner that is altogether disgraceful to the age. No uniform means have ever been taken to amend them; they are miserable compilations, or copies of each other, and the few correct soundings that have been here and there interpolated, instead of serving as standard points to adjust the rest, actually increase the general confusion,

The Admiralty has for some time very judiciously employed Mr. Tiarcks in determining the longitudes of several interesting places, by the mean of a multitude of chronometers. Would not a similar method, devoted, for a few summers, to the construction of an entirely new chart of these banks, be one of the most essential benefits that could be conferred on the navigation and commerce of our home seas?

A frigate or other convenient ship might carry the necessary instruments and chronometers, while the soundings should be taken by tenders, or in fine weather, and particularly in strong tides, by boats. The ship should go sufficiently off the wind to avoid

leeway as much as possible, her track should be slow but undeviating, and her constant observations for latitude and longitude should be connected by the perpetual log of Gough or Massey. The sounding vessels should move in parallel lines to the course of the centre ship, there might be three or four of them on each quarter, and their precise situation when the lead was at the bottom, and the line perpendicular, would be ascertained by their bearing, and the angular altitude of her mast-head.

The depth of the water, uniformly reduced either to the mean or the minimum of spring-tides, and the nature of the bottom, would be the two principal points of inquiry; but an excellent opportunity would be likewise afforded for learning the direction, duration, and combination of the tides in the offing, their real rise and fall at a distance from the land, and the influence of the Atlantic and Biscayan currents on the tides, as well as their united effect in transporting the various deposits from the rivers, by means of which a constant accumulation of sand or mud is produced in one place, while the rocky bottom is denuded in another. How far the temperature of the sea is affected by proximity to the land, or by the shoaling of the water, and to what cause is to be ascribed the mutable colour of the sea, which suddenly varies from light green to dark blue, are two amongst several other subjects of research which would well deserve the attention of the person employed on this service.

To preserve consistency in the terms used to describe the several substances found at the bottom, and which are now named with the most amusing caprice, such as crab's eyes, oat-husks, hake's teeth, *&c.*, every cast of the lead should be registered, and the arming cut off and numbered in like manner; or the sand might be separated from the tallow, washed, and folded up in papers. These, with all the other data, should be transmitted to the Hydrographical Office, where they might be laid out in their respective situations on the floor of a large room, graduated for that purpose, and where they would be easily grouped, so as to shew the general arrangement of the districts of the sands, gravel, shells, and stones.

The geologist would likewise find in this model (if it may be so called) of the mouth of the channel a most interesting subject for investigation;—he would trace the connexion between these substances and the several strata of the adjacent shores; he would determine whether the shells and other organic fragments are recent or fossile; and he would distinguish the predominant from the adventitious matter which currents and other accidental causes have strewed in some places to the frequent perplexity of seamen.

If these suggestions should be adopted by the present enlightened and active Board of Admiralty, it may be presumed that they would produce information of considerable value both to the philosopher and the sailor; but it is certain that they would lead to the formation of an accurate and rational submarine map of the channel, and thereby accomplish one of the most important desiderata of practical navigation. I am, Sir,

Your's, &c. B.

Art. IX. *Some particulars respecting the Ornithorhynchus Paradoxus. By* H. Scott, Esq.

I spent a week at Bathurst, in October 1820, (the commencement of the Australian spring,) and during my stay there I received from a young man, born in the colony, the following note, and also the female ornithorhynchus, to which it alludes:

Sir, *Oct.* 13.

The bearer, (a native black man,) is one of the men that came with me.

I yesterday evening went to shoot some ducks, and was fortunate enough to take a female platypus from her nest, of which I shall give an account when I see you, and was lucky enough to catch a pair of young swans, which I now send.

I am, Sir, your most obedient,

John Rowley.

I received the above about twelve o'clock the same day, Mr. Rowley being then at a lagoon about nine miles distant.

The animal was very lively, and I put it into a large tub of water, and fastened a string to its hind leg, that it might not escape in the night; this caused an inflammation, and the next morning it died.

I watched its motions during the whole of the afternoon, and questioned the native black, (through the medium of another who spoke English remarkably well,) as to its haunts and habits, and the following are my observations and his account.

It was extremely lively, and would attempt to bite when touched, but did not hurt.

The beak is soft and slimy; it dives down and rises again immediately, shaking its head and bill like a duck; it runs, or rather crawls, one foot before the other on the ground, somewhat fast; its excrement is soft and brown like that of a bird; it scratches its head and neck with its hind foot like a dog; its eye is very round, but the socket is oblong; the colour of the iris a dark brown, the pupil very minute and blue, rather a prussian blue; it breathes through one nostril, apparently, as if the water came from one only. In the evening it became rather more lively, but died the following morning, when Mr. Hill, a surgeon of the Royal Navy, who was with us, opened and dissected it, and wrote me a letter, which I have added to this account.

From Cook-a-Gong, chief of the Burrah-Burrah tribe, our guide, I learnt that this animal had just finished building her nest, which has a long niche or tube to creep into, and which leads to a round hollow, the whole lined with reeds and moss.

It is built amongst reeds in still water *; it lays two eggs at a time, their colour white, and their size that of a small sized hen's

* A few days afterwards I passed through this lagoon, and rode up to the nest, the waters being above my horse's knees. I found a large mass of reeds scratched and twisted together on the stump or root of one of the reeds; the canal had been nearly destroyed in getting the animal out, but the large hollow lined with moss was perfect, and appeared moist. She sat with her bill about an inch or two above the water for air; this in several other places, during a six weeks' tour in the interior, I found to be invariably the case in rivers, and the most usual mode of discovering them, which, in general, was difficult, for they are very quick-sighted and hy.

egg; it sits on them a long time, and hatches them like a fowl; it will not forsake its nest by being disturbed; it eats soft mud, but no grass or weed *.

They have been caught on dry ground some distance from the river.

When attacked the male strikes with his hind leg, which has a spur, (the female has none,) and the wound causes considerable swelling and pain, but no instance of death in consequence has been known. To cure the wound it is washed with cold water, and sucked by the natives, who call it mullongong.

Mr. Hill's Letter.

Dear Sir, *Sydney, January* 14, 1821.

I have sent you the preparation of the female ornithorhynchus, properly corked and sealed; and from the state of preparation it is now in, I am in hopes it will not require a change of spirits until your arrival in England: it would be well, however, to examine it occasionally on the voyage.

It consists of the internal organs of generation, the urinary bladder, with part of the ureter attached, a small portion of the rectum, and the whole of the external parts.

You will observe that the preparation shews:

1. One common external orifice for the rectum, vagina, and urinary bladder.

2. That the vagina, termination of the rectum, *&c.*, remain unexamined.

3. The urinary bladder.

4. The junction of the fallopian tubes in one common canal, (behind the bladder,) but without any organ or cavity, like the uterus of viviparous animals: at their junction in this canal the tubes are of thicker consistence, approaching a cartilaginous feel.

The urinary bladder, situated immediately before the common canal, must not be mistaken for the uterus.

* Mr. Hill had caught and opened a male a few days before, and found the stomach full of mud, and very offensive. The female I had was quite empty, and not offensive.

5. The fallopian tubes terminating in the ovaries.

6. The left ovary in an impregnated state, containing one ovum of the size of a small pea, round, and of a yellow colour; also two of smaller size, and a great number of minute vesicular bodies hardly perceptible to the eye, but very evident with a microscope.

7. The right ovary without any signs of impregnation.

8. Part of the rectum, to shew its attachment and termination.

The preparation of the spur * I intend sending to Mr. M'Leay, as he was the first that published an account of it, and has always supported the fact against a host of unbelievers †. If you have an opportunity of shewing him the other preparation, I am sure he will feel much gratified.

Your most obedient servant,

PATRICK HILL, *Surgeon*, R.N.

To —— —— ——

[We refer our readers to the third volume of Sir E. Home's *Lectures on Comparative Anatomy*, (p. 341,) for further details and observations respecting the anatomy and habits of this very singular animal.]

ART. X. *Proceedings of the Royal Society.*

THE Society held their first Meeting after the Christmas Vacation, on

Thursday, January 8,

when Anthony Story, Esq., was admitted a Fellow.

A paper was communicated by J. F. W. Herschel, and James South, Esqrs., entitled, "Observations on the apparent distances and positions of 380 double and triple stars, made in the years 1821, 1822, and 1823, and compared with those of other astronomers; together with an account of such changes as appear to have taken place in them since their first discovery."

* This is the spur from the male alluded to above.

† During our excursion I frequently asked the natives, whom we met, what were the effects of this spur? and they always made up piteous faces, and said, "murray bad," (very bad.) These accidents have happened also at Bathurst.

Thursday, January 15.

Messrs. J. H. Vivian and M. Faraday were respectively admitted Fellows of the Society.

The reading of Messrs. Herschel and South's paper, of which the following is an abstract, was resumed and concluded.

The determination of the apparent distances and positions of such double stars as could be measured with micrometrical instruments, and high magnifying powers, was suggested by Sir W. Herschel more than forty years ago; and in his hands it led to a new department of physical astronomy, by the discovery of sidereal phenomena, referable to the agency of attractive forces. But the determination of the existence of annual parallax, the immediate object for which the inquiry was instituted, was soon lost sight of, in the more extensive views of the construction of the Universe, which gradually unfolded themselves. Nor has the investigation been resumed, although from the precision with which such observations can be made, it seems in the opinion of the authors of this paper likely to be the mode by which the existence or non-existence of sensible parallax, will ultimately be determined.

The results of Sir William Herschel's observations from 1779 to 1784, were published in the *Philosophical Transactions* for 1782 and 1785; and a re-examination after a lapse of twenty years was undertaken by him in 1801-2-3 and 4; and in the *Transactions* for 1802 and 1804, unexpected phenomena were communicated. Instances in which two stars were performing to each other the offices of sun and planet were proved to exist, and to more than one pair the period of rotation was, according to the observations of the authors of this paper, assigned with considerable exactness. Immersions and emersions of stars behind each other had been witnessed, and real motions among some of them had been observed rapid enough to be detected, in very short intervals of time.

But as from the novelty of the subject, and from the imperfections of the micrometers employed in 1779 and 1780, it was likely hat some instances of error had occasionally crept in, it became

desirable that a second re-examination should be instituted: accordingly in the year 1816, some progress was made by Mr. Herschel towards its accomplishment, and the results are communicated in the present paper. A similar idea having, however, occurred to Mr. South, it was at length determined that the observations should be carried on in concert, and with his instruments.

Meanwhile (unknown to the authors of this paper) a similar undertaking had been entered upon by a distinguished continental astronomer, Mr. Struve, director of the Imperial Observatory at Dorpat, and the general coincidence between the measures of this observer and those of their own, is deemed at once interesting and corroborative of the accuracy of both.

The instruments with which the observations accompanying this paper were made, are a five and seven feet equatorial; the former was constructed under the direction of the late Captain Huddart, and is remarkable for its extreme lightness, for the promptitude with which it obeys its adjustments, and for its ability in retaining them. Its object glass $3\frac{3}{4}$ inches aperture, and of five feet focal length, is the work of the late P. and J. Dollond, whilst its divided circles, microscopes, *&c.*, were completed by Messrs. I. and E. Troughton. A description of it is given, and a drawing is annexed. The latter is a telescope of seven feet focal length, and five inches clear aperture; it was made by Tully, and is mounted on the polar axis of the old equatorial sector, made for the royal observatory, and for the use of which an acknowledgment is made to the Council of the Society.

The micrometers employed are the work of Mr. Troughton, and have long since been familiar to astronomers under the name of Troughton's Wire Micrometers. The measures of distance are all central.* The observations of each star were generally made in each other's presence; but occasionally in different parts of the observatory, and with different instruments, without any communication with each other; in some instances the observations of Mr. Troughton or Mr. Richardson have been appealed to, in order to settle discrepancies.

To the observations of each star the authors attach their mean

result: the results obtained by other observers are also placed in the order in which they were made; but there is one circumstance to which they solicit attention, namely, that as far as Sir William Herschel's observations are concerned, the dates and results will not accord with those published by Sir William in the *Transactions*, for reasons given in a former part of the paper.

As an Appendix, measures of a few stars less perfectly observed are added, which, although not entitled to equal confidence with the others, the authors think may, perhaps, still have their use.

Thursday, January 22.

Dr. Scudamore was admitted a Fellow of the Society.

The following paper was read:

On a Mode of preventing the Corrosion of Copper Sheeting by Sea Water, in Ships of War, and other Ships. By Sir H. Davy, Bart., P.R.S.

When copper sheeting, however pure the metal may be, is exposed to sea water, a green rust is formed upon it, which, when washed off, is replaced by a similar substance, till the whole of the metal is thus destroyed by corrosion. To prevent this effect, the President avails himself of the modification of chemical affinities, derived from electrical powers; and in pursuing his researches in relation to this subject, he found the above-mentioned action upon copper counteracted by very weak negative electricity easily excited in it by the contact of a surface of tin, not exceeding $\frac{1}{100}$ that of the copper, and made part of an electric circuit in sea-water. Other metals may be substituted, but the ease with which a perfect contact is made by solder with tin, and the facility with which its submuriate detaches from the metal, induced Sir H. to regard it as best adapted to the purpose. He observes further, that the cause which prevents the oxidation of the copper, will also probably prevent the adhesion of marine animals, and of vegetables. After adverting to the unequivocal and satisfactory results of his experiments made upon a small scale, the author states that the Lords

Commissioners of the Admiralty have enabled him to make arrangements for pursuing them on a very extended plan.

A paper was also read entitled,

Experiments and Observations on the Developement of Magnetical Properties in Steel and Iron, by Percussion. By W. Scoresby, Jun., F.R.S.E. communicated by the President.

After adverting to the general results of his former inquiries the author observes, that his principal objects on the present occasion were to endeavour by auxiliary rods of iron to increase the degree of magnetism, and to ascertain on what circumstances as to the magnitude of the iron rods, and the quality, size, and temper of the steel wires, the utmost success of the method depends.

He formerly used a single iron rod, upon which the *steel* bars were hammered, both being in a vertical position. He now places the steel wire between two rods of iron, and subjecting it through the medium of the upper rod to percussion, derives the advantage of the magnetism of both rods of iron acting at the same time upon both its poles. The rods he used were of the respective lengths of three and one foot, and an inch diameter, and the upper end of the larger rod and the lower one of the smaller rod were made conical, there being an indentation in each to receive the ends of the steel wire. Some magnetism was then elicited by percussion in the larger rod, and the steel wire being properly placed between its upper extremity and the lower one of the small rod, the upper end of the latter was hammered, and magnetism thus communicated to the wire; whilst the lower rod receiving some influence from the percussion, performed a similar office. The author calls this mode of proceeding the *compound process*, to distinguish it from the mere hammering of the wire upon the rod, as practised by him formerly, and which he terms the *simple process*. He then enters into extended details of his several experiments, of which the following are the principal results. 1. That the *compound process* is more effectual in the production of magnetism than the *simple* one, though the ratio of augmentation does not appear determinate. In one experiment the maximum effect of the simple process was an attractive

force capable of lifting between 186 and 246 grains, while the compound process augmented the lifting power to 326 grains. In another, the simple process gave a lifting power of 246, the compound of 345 grains. Moreover, the efficacy of the compound process is much less manifest upon long than short wires; and the softer the wire the more susceptible it becomes of this magnetic condition.

The author concludes this paper with some theoretical remarks respecting the influence of percussion in disposing the particles of iron to receive and retain magnetism, which he thinks may tend to explain some otherwise obscure phenomena, and which seem to render it probable that the process of percussion may be applied, in connexion with other modes of magnetising, for giving increased power to magnets.

Thursday, January 29.

Thomas Amyot, Esq. was admitted a Fellow, and the following paper was communicated.

Observations on the Iguana Tuberculata, the common Guana. By the Rev. Lansdown Guilding, B.A., &c., *communicated by* Sir E. Home, Bart., V.P.R.S.

The author's chief object in this communication is to correct an error into which many naturalists have fallen, of describing the gular process of some lizards as a pouch capable of inflation, and to point out a new organ on the parietal bones of the head of the Iguana, which he proposes to name *Foramen Homeanum*; it leads to the cavity of the brain, and is covered by a brown oval scale, semitransparent in the centre, but not affording a passage to any nerve or blood vessel.

A paper was also read, entitled

A finite and exact Expression for the Refraction of an Atmosphere, nearly resembling that of the Earth. By Thomas Young, M.D., For. Sec. R.S.

Having shewn that if the pressure of the atmosphere be repre-

sented either by the square or by the cube of the square root of the density, the astronomical refraction may be obtained in a finite equation; and having adverted to Mr. Ivory's computation of the refraction with the assistance of converging series, and several transformations from an equation which expresses the pressure in terms of the density, and of its square, Dr. Young proceeds to observe, that if we substitute for the simple density the cube of its square root, we shall represent the constitution of the most important part of the atmosphere with equal accuracy, although this expression supposes the total height somewhat smaller than the truth; and that we shall thus obtain a direct equation for the refraction which agrees very nearly with Mr. Ivory's table, and still more accurately with that in the Nautical Almanac, and with the French tables.

At the horizon the refraction is equal to 33′ 49″.5, which is only 1.5″ less than the quantity assigned by the French tables and in the Nautical Almanac, while Mr. Ivory makes it 34′ 17″.5. Again, for the altitude 5° 44′ 21″ we obtain 8′ 49″.5 for the refraction, while the Nautical Almanac gives us 8′ 53″, and Mr. Ivory's tables 8′ 49″.6. The author, however, observes that there is no ıeason for proceeding to compute a new table by this form u, since the method employed for that in the Nautical Almanac is, in all common cases, more compendious; and even if it were desired to represent Mr. Ivory's table by the approximation there employed, we might obtain the same results with an error scarcely exceeding a single second, from an equation of the same form.

Thursday, Feb. 5, and Thursday, Feb. 12.

At these Meetings

The BAKERIAN LECTURE—*On certain Motions produced in Fluid Conductors, when transmitting the Electric Current.* By J. F. W. Herschel, Esq., F.R.S., was read.

In the first paragraphs of this lecture Mr. H. particularly describes the phenomena that result on placing a portion of mercury, covered with sulphuric acid, between the voltaic poles immersed on opposite sides of the globule of metal, but in contact with the

acid only. They consist in active motions of those particles of the acid in contact with the mercury, while the superficial molecules of the metal continually radiate from the point nearest the negative pole, and darting to the positive pole, return along the axis. The author particularly notices several singular appearances resulting from this current, and shews them to be independent of any electro-magnetic vortices, to which at first sight they present considerable analogy; they are incomparably more forcible in proportion to the electric powers used, than the motions produced by the action of magnets. Hence they furnish an extremely sensible test of the developement of feeble voltaic powers, not easily rendered sensible by other means.

The author next describes the appearances observed in cases where other liquids and metals are used, and adverts to the influence of several causes upon the uniformity of the results. Among these, impurity in the mercury is especially noticed, which should not only be carefully distilled, but also well washed with dilute nitric acid. Mercury thus purified, and placed in the circuit as before, exhibits phenomena varying with the nature of the liquid. Generally speaking, currents are produced radiating from the point nearest the negative pole, which are most violent in acids, and less in saline solutions, in proportion as the electro-positive energy of the base is greater. In many liquids a counter current from the positive pole is observed; but if either pole be brought into contact with the mercury, no currents are observed from the point of contact, but strong ones are perceived to radiate from the other. If the negative pole touch, it amalgamates with the mercury, which remains bright; if the positive pole, the mercury rapidly oxidizes, and in both cases currents are produced.

Mr. H. proceeds to observe, that when mercury is electrized in saline solutions, its properties are generally altered, and he describes at length the phenomena thus presented in a solution of sulphate of soda, which were peculiar and apparently perplexing, but which he found to depend upon the presence of amalgam of sodium, counteracting the effect of the negative pole, and exalting that of the positive in proportion to its quantity, until it overcomes

and even reverses it. That sodium is actually present in these cases, the author shows by the following experiments:

Having detached the negative wire, he touched the mercury now lying quiet in the liquid with a platinum or copper wire, and a violent action instantly began. The mercury rushed to the wire in a superficial current, and it gave off abundance of hydrogen. The sodium, wire, and liquid, forming a voltaic combination sufficiently powerful to decompose the water.

The author next proceeded to investigate more minutely the effects of different metals in their contact and amalgamation with mercury, employing solutions of the caustic alkalies for the conducting liquids, which have the advantage of producing no currents in pure mercury, so long as neither pole is in contact with it.

In liquid potash a contact with the negative pole, of a single second's continuance, imparted to 100 grains of mercury the property of rotating violently from the positive to the negative pole, when the circuit was completed in the liquid alone. The rotation was even forcible when the quantity of potassium did not probably exceed a millionth part of the whole mass. With sodium similar effects were observed, and even when the proportion of sodium to mercury was only as 1 : 1.600.000, a feeble motion was sensible.

The influence of barium, strontium, calcium, and magnesium, and of zinc, lead, tin, and iron, is next described, the alloys of these metals being all possessed of the positive property. Copper, on the other hand, does not communicate it, though present in considerable proportion; nor do bismuth, silver, or gold.

Mr. Herschel concludes this lecture with some general and theoretical observations and deductions, founded on his experimental inquiries. These relate principally to the exceedingly minute proportions of extraneous matter, capable of communicating sensible mechanical motions and properties of a definite character to the body they are mixed with. When we see energies so intense exerted by the ordinary forms of matter, we may, says the author, reasonably ask, what evidence we have for the imponderability of any of the powerful agents, to which so large a part of the activity of material bodies seems to be owing?

Among the essential conditions of the phenomena, the author particularly adverts to the vast difference of conducting power between the metallic bodies set in motion, and the liquid under which they are immersed; to the necessity of the perfect immiscibility of the conducting fluids, so as to render the transition from one to the other quite sudden; and to a certain chemical or electrical relation between them. Under these conditions, Mr. H. observes, the phenomenon may admit of explanation, from what we already know of the passage of electricity through conductors, and the high attractive and repulsive powers of the two electricities *inter se.* A body so highly electro-positive as potassium present in mercury may, for instance, have its natural electrical state exalted by its vicinity to the positive pole; and being thus repelled, *may* take the only course the resistance of the metal on the one hand, and attraction of cohesion on the other, will permit, *viz.*, along the surface, to recede from the positive pole. It may even act as a carrier to the positive electricity, *which may* adhere to it too strongly to be transmitted through the mercury, and when arrived at the opposite side of the globule may then, by the influence of the opposite pole, lose its exalted electrical state. Such an explanation, however, is not without its difficulties, and although another course is open to us, that of considering the action which takes place at the common surface of two unequally conducting media, as dependent on a new power of the electric current, bearing some analogy to magnetic action, yet this, in the present state of the investigation, must be regarded not only as a bold, but vague hypothesis.

Thursday, Feb. 19.

A Paper was read

On Semi-decussation of the Optic Nerves. By W. H. Wollaston, M.D., V.P.R.S.

In the human brain, the optic nerves, after passing forward to a short distance from their origin in the thalami, become incorporated, and from the point of union two nerves are sent off, one to each eye. To this united portion the term *decussation* has been applied,

under the supposition that, though the fibres do intermix, they still continue onward in their original direction, and that those from the right side cross over wholly to supply the left eye, while the right eye is similarly supplied by fibres from the left thalamus. Anatomists have considered this opinion as confirmed by the circumstance of the nerves actually crossing each other as two perfectly distinct cords in certain fish. The author, however, from a species of blindness under which he has more than once suffered, concludes that a different distribution of the nerves takes place in the human subject. This peculiar state of vision consisted in seeing only half of every object, the loss of sight being, in both eyes, towards the left, and of short duration only. In reflecting upon this subject a certain arrangement of the optic nerves, not consistent with the generally received hypothesis of their decssation, occurred to him. Since the corresponding points of the two eyes, he observes, sympathize in disease, their sympathy is evidently from structure, and not from mere habit of feeling together. Any two corresponding points must be supplied with a pair of filaments from the same nerves, and the seat of a disease in which similar parts of both eyes are affected, must be considered as situated at a distance from the eyes, at some place in the course of the nerves where these filaments are still united, and probably in one or other thalamus. It is plain, therefore, that the cord which comes finally to either eye, under the name of optic nerve, must be regarded as consisting of two portions, one half from the right thalamus, and the other from the left. Upon this supposition decussation will take place only between the adjacent halves of the two nerves. That portion of nerve which proceeds from the right thalamus to the right side of the right eye, passes to its destination without interference; and in a similar manner the left thalamus will supply the left side of the left eye with one part of its fibres, while the remaining halves of both nerves, in passing over to the eyes of the opposite sides, must intersect each other, either with or without intermixture of their fibres.

Dr. W. observes that the crossing of the nerves to the opposite eyes in fish, is in conformity with this view of the arrangement of

the human optic nerves; for in the sturgeon, for instance, the eyes are placed so exactly back to back, that there are no corresponding points of vision requiring to be supplied with fibres from the same nerve. In this animal an injury to the left thalamus might be expected to occasion entire blindness to the right eye alone; in ourselves a similar injury would occasion blindness to all objects situated to our right, owing to insensibility of the left half of the retina of both eyes. Dr. Wollaston states some other facts, illustrating his view of this peculiar distribution of the human optic nerves, remarking that in common vision also the sympathy of corresponding points, which receive similar impressions from the same object, is dependent upon the same arrangement of nerves, to which the term *semi-decussation* may be applied. In conclusion, he observes that, so long as our consideration of the functions of a pair of eyes is confined to the performance of healthy eyes in common vision, when we remark that only one impression is made upon the mind, though two images are formed on corresponding parts of the retina, we may rest satisfied in ascribing the apparent unity of the impression to habitual sympathy of the parts. But when we regard sympathy as arising from structure, and dependent on connexion of nervous fibres, we therein see a distinct origin of that habit, and have presented to us a manifest cause why infants first begin to give the corresponding direction to their eyes; and clearly gain a step in the solution, if not a full explanation, of the long agitated question of single vision with two eyes.

Thursday, February 26.

A Paper was read, entitled

Experimental Inquiries relative to the Distribution and Changes of the Magnetic Intensity in Ships of War. By Geo. Harvey, Esq.

This paper contains the details of a number of experiments made on board several vessels, with a view of determining the influence of the iron in the ships upon the compass under different circumstances and situations. The instruments used for determining the intensity consisted of a magnetized cylindrical bar 2.5 inches long, and $\frac{3}{80}$ inch diameter, delicately suspended by

a single fibre of the silk worm to the extremity of an adjusting screw, which worked in the cap of the glass vessel enclosing the bar. A brass wire also passed through the cap for the purpose of placing the bar at right angles to the magnetic meridian previous to its being put into a state of oscillation.

On the days devoted to the experiments on ship-board, the time of making 50 vibrations of the bar was determined in the centre of a meadow, of which the substratum was clay-slate, by a mean of 6 sets of experiments, the time being accurately registered to quarter seconds. The instrument was then taken on board, and placed in succession at the different stations in the ship, and the mean of 6 sets of experiments determined at each station, with the same precaution as on land. The time, says the author, of performing the oscillations on shore, and at each of the assumed points in the ship, necessarily gave the magnetic intensity at each station in terms of the terrestrial intensity, which in this case was represented by 100.

Thursday, March 4.

William Wavell, M.D., and Captain Philip Parker King, R. N., were admitted Fellows.

At this Meeting of the Society a Letter from Sir E. Home, addressed to the President, was read, containing

Some curious Facts respecting the Walrus and Seal, discovered by the examination of Specimens brought to England in the different Ships lately returned from the Polar Circle.

The first fact stated by Sir Everard Home in this paper is, the analogy in structure between the hind foot of the walrus and the foot of the fly. In both there is a very similar apparatus for producing a vacuum, so as to enable the animal to proceed upon smooth surfaces against gravity by the adhesion of the feet thus effected, there being 2 cups in the foot of the fly, and one in that of the walrus for this purpose. Secondly, he notices the peculiar mode in which the bile in the walrus is collected in a reservoir, and thence forcibly impelled into the duodenum.

The third new fact which the author adduces is the peculiar structure of the funis and placenta of the seal. In this animal the vessels forming the funis are not twisted; their whole length is 9 inches. Three from the placenta, they give off anastomosing branches, connected with it by three membranous folds, between which the blood vessels are conveyed to the placenta. This structure gives uncommon facility to the placental circulation, and makes it worth inquiry whether the same peculiarities exist in other marine animals. Several illustrative drawings accompany this paper.

On the same evening a Paper was communicated, entitled *Further Particulars of a Case of Pneumato-Thorax*. By J. Davy, M.D. F.R.S.

About a month after the operation described in Dr. Davy's former paper, when the patient appeared to be doing well, symptoms of hydro-thorax came on, and fluid again collected in the left side of the chest;—a second operation therefore was performed, and 14 ounces of fluid discharged through a perforation in the fifth rib. During the six following weeks not less than 20 pints of fluid ran off through the opening—at first it was transparent, but became gradually more and more purulent, and was mixed with air composed of oxygen, azote, and carbonic acid, in various proportions. The patient's health improved at first progressively, but in about 6 weeks after the operation he became worse, and expired suddenly. On examination after death about 6 oz. of pus were found in the left pleura. The right pleura was healthy, but tubercles and vomicæ were found in the right lung. The left lung was much condensed, and communicated by two small openings with the pleura. Dr. Davy referred the origin of the disease in this case to a communication between the aspera arteria and cavity of the pleura, established by the rupture of a superficial bronchial tube and the membrane covering it; he concluded the paper with some remarks upon the fluctuation and composition of the air from the chest, which he attributed not to the varying quantity of atmospheric air admitted through the per-

foration, which was as carefully closed as possible, but to its vitiation by respiration, and by the absorbent power of the pleura.

Thursday, March 11.

A paper entitled *Remarks on the Parallax of α Lyræ.* By J. Brinkley, D.D. F.R.S., *&c. &c. &c.* ,was read.

The author's object in this paper was principally to form a correct estimate of the absolute and relative degrees of accuracy of the instruments at Dublin and at Greenwich. He first considered the difference of parallax between γ Draconis and α Lyræ, and secondly the absolute parallax of α Lyræ.

He exhibited in a table the whole of the results of 337 observations of Mr. Pond for the intercepted arc, reduced to 1 January, 1815; chiefly by Mr. Pond's own computations. From 46 of the observations, made in the year 1812, he deduced 0″.28 for the coefficient of the effect of parallax: and from such of the observations as were made in the same day the number deduced is 0″.54.

In 1813 there was a difference of half a second between the mean of 22 observations in June and July, and of 17 in August; hence Dr. Brinkley was led to examine the observations of this year alone, and he found that 61 of them from June to December, as reduced by Mr. Pond, gave 0″.42 for the co-efficient of parallax: and omitting the last 5 days of observation 0″.89, which is little less than the result of his own researches.

On the other hand, when 5 double observations, in January and February, 1814, were added to these 61, they reduced the result for the co-efficient to 0″.18. So that the discordances seem to be too great to enable us to place any reliance on the conclusions respecting the actual magnitude of the annual parallax.

A similar fluctuation is observable in the results obtained for the following years: and though it might, on the whole, be inferred that the parallax is about ⅔ as great as that which the author has assigned from his own observations, yet he contents himself with concluding that the mural circle of Greenwich has *not* sufficiently proved the identity of the distance of the two stars

in summer and winter within one-tenth of a second: but, on the contrary, that it shews the parallax of α Lyræ to be half a second greater than that of γ Draconis.

In 1815, the first 15 summer observations, compared with the first 13 in November, give a parallax of $+ 0''.72$; the next 16 in summer, compared with the next 16 in winter, give a *negative* parallax of $- 0''.58$; a comparison which sufficiently proves the imperfection of the observations, depending probably on an unsteadiness in the instrument.

In the whole five years the mean of all the observations in August exceeds the mean of July by $0''.51$; a discordance which parallax would diminish but in an inconsiderable degree.

The author pursued a similar train of argument in the second part of the inquiry, relating to the absolute parallax of α Lyræ. While the circle at Dublin, he observes, made from a mean of several years the double zenith distance of this star $3''$ greater in the beginning of December than in the beginning of August, that of Greenwich shews no difference whatever in the double altitude observed by reflection in summer and winter. There are, however, differences of above 4 seconds in the difference of altitude of Lyra and of the Pole-star, as determined in different years by the same instrument: and Dr. Brinkley observed, that an unsteadiness, amounting to $15''$ or $20''$, is discoverable in the comparative results of the different microscopes; whence he infers that there must be an uncertainty, amounting to many tenths of a second, in the mean.

The co-efficients of aberration and of solar nutation, which come out $20''.35$ and $0''.51$, are certainly true to $\frac{1}{4}$ or $\frac{1}{10}$ of a second, as deduced from the observations of Dublin: the author thinks it therefore fair to infer that $1''.14$, the co-efficient of annual parallax for α Lyræ, is correct nearly in the same proportion. Nor are there any changes from season that could produce the appearance of regular parallax of all the stars in which it has been inferred: and it is very improbable that any error of the instrument could have given a parallax to Lyra, and left the Pole-star completely free from it.

The last of the tables shew the consistency of the circle of Dublin in the places of the stars as determined by it after the interval of a considerable number of years, without any such tendency to the south, as is supposed to have been observed at Greenwich.

Thursday, March 18.

An Account of Experiments on the Velocity of Sound, made in Holland, by Dr. G. Moll *and* Dr. A. Van Beck, was communicated to the Society.

After noticing the difference between the celerity of sound, as deduced by theory, and found by experiment, and La Place's explanation of the cause of that difference, and his corrections of the Newtonian formula, the authors proceeded to consider the influence of the variable force of wind upon its velocity, and state their mode of annihilating such cause of error. They then detailed their own experiments, for which they selected two open and elevated spots in the plains of Utrecht, distinctly visible from each other, and distant about 96.64 fathoms: they measured the interval between seeing the light and hearing the sound, by clocks, with conical pendulums, which divide the 24 hours into 10 million parts, and one of the indexes of which give $\frac{1}{100}$ part of a decimal second. Each station was also furnished with a good barometer, several accurate thermometers and excellent telescopes, and the humidity of the air was determined by Daniell's hygrometer. The authors then described the means which they adopted to ensure the simultaneous firing of the shots at both stations, and by which they succeeded in bringing them within 1″ or 2″ of each other, and entered at considerable length into the details of their different experiments, the results of which are given in several tables annexed to this paper, among which will be found one, exhibiting a general view of the results of the experiments of those different philosophers who have investigated this subject.

In conclusion, it appears from their researches that at the temperature of 32° the velocity of sound is 1089.7445, English feet per sexagesimal second.

At this meeting the Lord Bishop of Limerick was admitted a Fellow of the society.

Thursday, March 25.

Major-General Sir John Malcolm, G. C. B., was admitted into the society.

A Letter from the Rev. L. W. Dillwyn *to* Sir H. Davy, Bart. P.R.S. was read.

This letter was supplementary to a former one, and contained further observations on the relative periods at which the different families of testaceous animals appear to have been created, and on the gradual approximation which may be observed in British strata, from the fossil remains of the oldest formations to the living inhabitants of our present land and waters.

The author observes that the dimyairia of the strata between the transition lime and lias have the ligament external, and that internal ligaments were therefore confined to the monomyairia till after the deposition of the lias.

In the beds above the lias, all the shells are referable to existing orders of animals, and it is only in the tertiary beds that any of the cirrhipeda, or families of the naked mollusca have been found.

What is generally considered as the beak of a sepia, Mr. Dillwyn refers to the cephalopode animal of an ammonite. Every shell of the tertiary strata, the author observes, may be referred to some existing genus; but though this approximation has thus far proceeded in the London clay, yet its numerous species are now extinct, and it is only in the upper beds of crag that any fossil can be completely identified with a living species.

A letter from Mr. Tredgold to Dr. Thomas Young was also read, containing,

An Account of his Experiments on the Elasticity and Strength of hard and soft Steel.

The bars of steel used in these experiments were supported at the ends by two blocks of cast iron, resting upon a wooden frame,

and a scale for weights was suspended from the middle of the length of the bar, by a cylindrical steel pin, $\frac{3}{8}$ inch diameter. To measure the flexure, a quadrantal piece of mahogany was attached to the frame with a vertical bar sliding in two guides at its edge, and moving an index. The bar and index were so balanced, that one end of the bar bore with constant pressure upon the specimen, and the graduated arc was divided into inches, tenths, and hundredths, and thousandths were measured by a Vernier. Abar of blistered steel, of file hardness, 13 inches long between the supports, underwent no permanent alteration of form when loaded with 110lbs. The temper of the bar was then successively lowered, and it was ultimately again hardened, but in these different states its flexure and resistance to permanent change of form remained the same. These experiments were repeated with bars of other dimensions, which were loaded till they broke, and from them the author also infers that the elastic force of steel is not altered by temper, and that the force which produces permanent alteration is to that which causes fracture in hard steel, as 1 : 1.66, and in the same steel, of a straw-yellow temper, 1 : 2.56. From comparisons of the strain required to cause permanent alteration in different kinds of steel, the author concludes, that in the process of hardening, the particles are put into a state of tension among themselves, which lessens their power to resist extraneous force, and the phenomena of hardening may be referred to the more rapid abstraction of heat from the surface of the metal, than can be supplied from the internal parts; whence a contraction of the superficial parts round the expanded central ones, and a subsequent shrinking of the latter, by which the state of tension is produced.

Thursday, April 1, *and Thursday, April* 8.

The following papers were read:

A Comparison of Barometrical Measurement with the Trigonometrical Determination of a Height at Spitzbergen. By Capt. Edward Sabine, F. R. S.

The hill selected for this comparative measurement was the highest within convenient distance, of which the ascent was prac-

ticable, on the western part of the N. coast of Spitzbergen. The summit was less than two miles from the observatory, in a direction nearly due south; the observatory being upon an island rather more than a mile from the main land. In consequence of the extreme inaccuracy of the plan of Fairhaven, published in Captain Phipps' voyage, the author annexed to this paper a sketch of the harbour and adjacent coast, to shew the positions of the hill and observatory. The small bay formed by the shore of the main land to the north eastward of the hill being frozen over, afforded a perfectly level base, and corrections for inequality were thus rendered unnecessary. A polished copper cone was fixed upon a staff at the summit of the hill, the apex of which was proposed as the height to the measured; it stood 44 inches above the highest pinnacle of the summit. Captain Sabine then entered into the details of this trigonometrical measurement, from which the altitude of the cone is considered as = 1644 feet. The author next proceeded to detail the particulars of the barometrical measurement, and the precautions taken to ensure accuracy in the instruments, and in their employment; and the height of the cone, thus ascertained, was 1640.07 feet.

Captain Sabine concluded this paper with some remarks upon the incorrectness with which the heights of the hills on this coast are set down in Captain Phipps' voyage.

An Inquiry into the nature of the luminous power of some of the Lampyrides; namely, the L. Splendidula, L. Italica, and L. Noctiluca. By T. J. Todd, M. D. *Communicated by* Sir E. Home, Bart. V. P. R. S.

After adverting to the various opinions entertained respecting the luminous powers of these insects and to some of the more usual phenomena which attend the emission or production of their light, the author proceeded to describe their structure, especially in relation to their luminous organs. The peculiar matter in which the power of emitting light appears to reside, is adhesive, semi-transparent, and granulated. According to Macaire, it is thickly penetrated by nervous filaments, and loses its luminous property

when broken down. The longest period which the author has observed the amputated organ to continue luminous is 20′, and it continues to shine in media of very different properties, in vacuo, under mercury, in water, and in oil. The light is re-excited by certain irritants; by heat and cold, by friction and by galvanism, by alcohol, camphor, and ammonia. In the living animal, also, mechanical and chemical stimulants excite the appearance of the light provided they do not disorganize the part. When the animals are killed by alcohol, tincture of hellebore, or of nux vomica, and certain other poisons, after all light and life have ceased, another fixed and steady light appears in the organ, varying in duration from 12 hours to 4 days. From the general results of his observations, the author concludes that the luminous powers of these insects are exclusively referable to vital action, and that their use has not been accurately ascertained, though probably connected with sexual distinction.

Sir F. Shuckburgh, Bart. was admitted a fellow.

The society then adjourned over two Thursdays, to meet again on the 29th of April.

Thursday, April 29.

The Rev. Dr. Maltby and E. H. Lushington, Esq. were admitted Fellows.

A letter from Dr. Tiarks to Dr. Young was read, containing

A short Account of some Observations made with Chronometers, in two Expeditions sent out by the Admiralty, at the recommendation of the Board of Longitude, for ascertaining the Longitude of Madeira and of Falmouth.

Dr. Tiarks was sent out to Madeira, in the year 1822, with fifteen chronometers, of which the rates had principally been ascertained in the Royal Observatory at Greenwich; he touched at Falmouth both in going out and in returning; and having again ascertained the rates of his time-keepers, he was thus enabled to obtain two distinct determinations of the longitude of Falmouth, which differed about four seconds of time from that which had been

inferred from the trigonometrical survey of Great Britain. It became, therefore, desirable that some further operations should be undertaken for the removal or elucidation of this discordance, and the following year a similar method was adopted with twenty-five chronometers, for determining the difference of longitude between Falmouth and Dover; this latter station having been chosen as easy of access, and as being perfectly determined; and the computations were made by interpolation, without employing any other rates for the chronometers than those which were observed in the different trips while they were actually on board of the ship; and latterly, when Dover Roads became unsafe, the operations were limited to the distance from Portsmouth to Falmouth: thus between the months of July and September the observations were made three times at Dover, four times at Falmouth, and three times at Portsmouth: and the comparison of their results affords a correction of five seconds of time for the difference of longitude of Dover and Falmouth, and of three for the difference of Falmouth and Portsmouth, agreeing completely with the error of four seconds attributed, from the observations of the preceding year, to the difference of longitude of Falmouth and Greenwich.

Hence Dr. Tiarks thinks it fair to conclude, that the diameter of the parallel circle on which the longitude is measured has in the survey been taken somewhat too great, and consequently the earth's ellipticity greater than the truth. He remarks, that the measurement of the spheroidical triangle concerned, determines only the actual flatness of the part of the earth's surface on which it is situated, and not the actual magnitude of the whole parallel, unless its curvature be supposed perfectly uniform, which we cannot assume with confidence: while, on the other hand, if we compute the ellipticity from the result of the chronometrical determination, it becomes $\frac{1}{314}$ instead of $\frac{1}{150}$, and agrees with the most accurate measurements obtained from different principles. The longitude of Falmouth is finally determined to be 20 minutes, 11.1 seconds of time, and that of the British Consul's garden at Funchal, 1 hour, 7 minutes, 39 seconds, west of Greenwich.

Thursday, May 6.

Lieutenant Henry Forster, R.N., was elected a Fellow of the Society, and being about to leave England on the Polar Expedition under Captain Parry, he was immediately admitted by the President.

The following papers were read :—

On Univalves. By Charles Collyer, Esq. *Communicated by* Sir James Macgrigor, F.R.S.

In this paper the author entered into a series of details respecting the structure of shells, and the anatomy of their inhabitants, which he thinks suggest the necessity of separating the natural history of the former from that of the latter. By parts peculiar to univalves, he proposed to distinguish and nominate families, to divide into sub-genera such as are distinguished by an uniform state of the more general feature, and to separate into individuals, such as with this particular state have additional parts, or modifications of such parts. To render the nomenclature perspicuous, he suggested Latin derivations of one termination, expressing some essential distinctive feature, or difference of colour or size; and where these fail, he had recourse to similitudes with other objects. The parts or conditions chosen for generic distinction and denomination, are cavity, lip, columella, rostrum, and spire, open, tubular. The application of these principles of distinctive description was illustrated by reference to several individuals, such as Argonauta, Cypræa, Conus, Trochus, and others. Shells that are partly or completely open and flat present, said the author, no feature for association, and hence a condition must be chosen, namely, the presence or absence of a margin. Lastly, the author divided tubular shells into straight and open, straight and closed, and contorted.

Of the effects of the density of Air on the Rates of Chronometers. By George Harvey, Esq. *Communicated by* D. Gilbert, Esq., F.R.S.

Among the sources of error to which chronometers are liable,

the effect of the variable density of the medium in which the balance vibrates has been overlooked, the author, therefore, proposes to investigate the effects of diminished and increased pressure, of transference from one to the other, and of the ordinary variations of atmospheric density upon the rates of chronometers.

In respect to diminished pressure he found that chronometers gained by being placed in air of less density than that of the ordinary state of the atmosphere, and that on the other hand they lost when subjected to air of greater than ordinary density. These experiments were made with a variety of chronometers, placed in the receiver of an air pump, or in that of a condensing apparatus.

In respect to the influence of ordinary changes in the density of the air, the author remarks that pocket chronometers are more readily affected than box chronometers, but that they all exhibit an increased rate under diminished density, and *vice versâ.*

The author shews that these changes in the rates, as observed in the air-pump and condensing apparatus, are independent of the changes of temperature resulting from changes in the density of the air thus rapidly effected, and therefore proceeds to inquire into the actual cause of the changes which his experiments indicate; he refers them to an increase in the arc of vibration when the density is diminished, and to a diminution in the arc, under increased density.

Thursday, May 20.

The Rev. Baden Powell was admitted a Fellow.

A Letter was read

To the President from Professor Berzelius, *dated Stockholm, April* 21, 1824,

In this letter Professor B. announces his discovery of a combination in a mineral, of chloride and oxide of lead, in the proportion of one atom of the former to two of the latter. He states that he has verified Mr. Phillips's researches on uranite, and gives a somewhat detailed account of his experiments on fluoric acid, and of the properties of the base of silica, which he procured by

heating potassium with dry fluosilicate of potassa, by which a silicuret of potassium and fluate of potassa are formed ; and which, when thoroughly washed with water, leaves a residue of hydroguret of silicium. It burns imperfectly in air and oxygen, at a red heat; is of a brown colour, and is acted upon by no acid except the fluoric. Silicium is readily oxydized when heated with carbonate of potassa or of soda, 100 parts producing 208 of silica. It burns in the vapour of sulphur, producing a white substance, which yields sulphuretted hydrogen when thrown into water, and silica remains in solution. In chlorine silicium burns at a red heat, and yields a colourless liquid chloride, the odour of which resembles that of cyanogen. In its nascent state silicium appears to combine with the metals.

This letter concludes with some remarks upon zirconium, obtained by the action of potassium on the alkaline fluate of zirconia; it is black, feebly acted upon by nitro-muriatic acid, soluble in sulphuric acid, and readily so in fluoric, with the disengagement of hydrogen. It unites with sulphur and chlorine, and readily burns in the air at a heat below redness.

Thanks ordered.

Thursday, May 27.

A paper by Dr. Wollaston was read,

On the apparent Direction of the Eyes in a Portrait.

Our account of this paper must necessarily be very imperfect, for want of the very curious and interesting drawings which accompanied it. Dr. W. observed that when we consider the precision with which we commonly judge whether the eyes of another person are fixed upon ourselves, it is surprising that the grounds of such judgment are not distinctly known, and that most persons in attempting to explain the subject would overlook some of the circumstances by which they are generally guided. Though it may not be possible to demonstrate by any decisive experiment on the eyes of living persons what those circumstances are, we may find convincing arguments to prove their influence, if it can be shewn, in the case of portraits, that the same ready decision that

we pronounce on the direction of the eyes is founded in great measure on the view presented to us of parts which have not been considered as assisting our judgment.

Dr. W. then adverted to the influence of the form of the iris as announcing the direction of the eye in portraits, and to that of the variable portion of the white shewn when the eye is variously directed in living persons; he remarked, however, that even in real eyes we are not guided by this circumstance alone, but are unconsciously aided by the concurrent position of the face; and he illustrated this opinion by reference to a series of drawings above mentioned, shewing that the apparent position of the eyes is powerfully influenced by that of the adjacent parts of the face, especially those which are most prominent: and these considerations are not limited in their application merely to cases of lateral turn of the eyes or face, but the same principles also apply to instances of moderate inclination of the face upwards or downwards; for when the face is directed downwards, the eyes that look at us must be turned upwards from the position of the face to which they belong; and if to eyes so drawn an upward cast of features be substituted for the former, the eyes immediately look above us.

From these and other details given in the paper, the author concludes that the apparent direction of the eyes to or from the spectator depends upon the balance of two circumstances combined in the same representation; namely, 1. The general position of the face presented to the spectator. 2. The turn of the eyes from that position; and thence proceeds to examine why, if the eyes of a portrait look at the spectator placed in front of the picture, they appear to follow him in every other direction. When two objects are seen on the ground at different distances from us in the same direction, one appears and must be represented exactly above the other, so that a vertical plane from the eye would pass through them, and since such a line will be seen upright, however far we move to one side, it follows that the same objects still seem to be in a line with us exactly as in

the front view, seeming, as we move, to turn from their first direction.

In portraits, the permanence of direction, with reference to the spectator, and corresponding change of its apparent position in space when he moves to either side, depends upon the same principles. The nose drawn in front with its central line upright continues directed to the spectator though viewed obliquely; or if the right side of the nose is represented, it must appear directed to the right of the spectator in all situations; and eyes that turn in a due degree from that direction toward the spectator, so as to look at him when viewed in front, will continue to do so when viewed obliquely.

On the same evening was read,

New Phenomena caused by the Effects of Magnetic Influence. By Mr. Abraham.—*Communicated by* Mr. W. Tooke, F.R.S.

In this paper Mr. A. detailed a series of experiments upon the passage of electricity through magnetized steel bars, which lead him to conclude that they possess a much better conducting power than the same bars in their common state, and consequently that they are better adapted for the preservation of buildings from lightning. On bringing one point of a magnetic discharging rod to the negative side of a charged jar, and presenting the other to the positive ball, he observed a deep red light between them, which he ascribes to the contact of the condensed magnetic and and electric atmospheres surrounding the ball and point.—Mr. Abraham concluded his paper with some observations upon certain atmospheric phenomena, especially relating to the Aurora Borealis, which he is inclined to ascribe to the joint influence of the electric and magnetic powers.

Thursday, June 3.

Dr. John Thomson of Edinburgh, was elected into the Society. Charles Lemon, Esq., was admitted a Fellow.

A Paper *on the Generation of Fishes*, by Dr. J. L. Prevost, was read.

The principal object of this paper was to describe the devolopement of the fœtus of the bull's-head, or miller's-thumb (*cotus gobio*).

The testicles are composed of a congeries of small canals, terminated at the upper part by cœca, and containing the semen, which they discharge into a common canal opening into the meatus, by which the urine is discharged. Under the microscope the semen appears composed of globules and animalcules. The eggs of the female are emitted covered with mucus, which swells up when it absorbs water. The yolk is enveloped in a fine membrane, adhering to which is a white granulated cicatricula, not visible before fœcundation.

The description of the developement of the fœtus given by the author is not intelligible without the annexed plate.

The Society then adjourned over one Thursday, to meet again on

Thursday, June 17;

at which meeting Lovell Edgeworth, Esq., was admitted a Fellow, and the following papers were read:

On the Action of finely-divided Platinum on Gaseous Mixtures, and its application to their Analysis. By W. Henry, M.D., F.R.S.

In the first section of this paper the author described the action of finely-divided platinum, at *common temperatures*, on mixtures of hydrogen and olefiant gas with oxygen; of hydrogen and carburetted hydrogen with oxygen; of hydrogen and carbonic oxide with oxygen; of hydrogen and cyanogen with oxygen; of carbonic oxide and carburetted hydrogen with oxygen; of hydrogen, carburetted hydrogen, and carbonic oxide with oxygen; and of the same with the addition of olefiant gas. From the experiments detailed under these several heads, it appears that when the compound combustible gases mixed with each other, with hydrogen, and with oxygen are exposed to platinum balls or sponge, the several gases are not acted upon with equal facility, but that, next

to hydrogen, carbonic oxide is most disposed to unite with oxygen; then olefiant gas, and lastly carburetted hydrogen. By due regulation of the proportion of hydrogen, the author remarks that it is possible to change the whole of the carbonic oxide into carbonic acid, without acting on the olefiant gas or carburetted hydrogen; he observes, however, that, with respect to olefiant gas, this exclusion is attended with some difficulty, and it is generally more or less converted into carbonic acid and water.

The second section of this paper related to the action of finely-divided platinum upon gaseous mixtures at *increased temperatures.* In these experiments the gases mixed with oxygen enough to saturate them, were severally exposed in small retorts containing a platinum sponge, and immersed in a mercurial bath to a temperature which was gradually raised till the gases began to act on each other. It was thus found that carbonic oxide began to be converted into carbonic acid at about 300°; olefiant gas was decomposed at about 500°; carburetted hydrogen at a little above 555°; and cyanogen appeared to require a red heat.

Muriatic acid mixed with half its volume of oxygen began to be acted upon at 250°; and ammoniacal gas, with an equal volume of oxygen, at 380°.

Adverting to the property inherent in certain gases of retarding the action of the platinum when they are added to explosive mixtures of oxygen and hydrogen, Dr. Henry observed that it is most remarkable in those which possess the strongest attraction for oxygen, and that it is probably to the degree of this attraction, rather than any agency arising out of their relations to caloric, that we are to ascribe the various powers which the gases manifest in this respect.

Dr. Henry concluded this communication by pointing out the best methods of analyzing mixtures of the combustible gases in unknown proportions.

An Account of the Organs of Generation of the Mexican Proteus in a developed state. By Sir E. Home, Bart., V.P.R.S.

The specimens described in this paper were taken in the month

of June, in a lake three miles from Mexico, at an elevation of 8,000 feet above the level of the sea; the usual temperature of the lake is 60°, and they are in such abundance as to form a principal article of food of the peasantry.

By the assistance of a series of annexed drawings by Mr. Bauer, Sir Everard fully describes the male and female organs of these animals, and is enabled to decide that they are a full grown and perfect tribe. "The attack therefore," says the author, "made upon Mr. John Hunter's sagacity, by M. Rusconi, in his work *Sur les amours des Salamandres aquatiques*, retorts upon himself."

On the Effects of Temperature on the Intensity of Magnetic Forces, and on the Diurnal Variation of the Terrestrial Magnetic Intensity. By S. H. Christie, Esq. M. A.

The details of the author's experiments upon the above subjects are given in an extended series of tables. Commencing with a temperature −3° F. up to 127°, Mr. Christie found, that as the temperature of the magnets increased, their intensity diminished, in direct contradiction to the notion of destroying magnetism by intense cold. From a temperature of 80° the intensity decreased rapidly as the temperature increased, and at above 100°, a portion of the power of the magnet was permanently destroyed.

Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of the Copper Sheathing of Ships, and to other Purposes. By Sir H. Davy, Bart. P.R.S

Since his former communication, the President has had an opportunity of pursuing his researches upon the above subjects, upon an extended scale, and with results perfectly conclusive and satisfactory. He found that sheets of copper defended by from $\frac{1}{100}$ to $\frac{1}{250}$ part of zinc or iron, exposed for many weeks to the full flow of the tide in Portsmouth harbour, suffered no corrosion, and that even $\frac{1}{1000}$ part of cast iron exerted great protecting influence.

Boats, and the sides of ships, protected in this way, were also similarly preserved.

Of the different protecting metals, cast iron is most convenient, and the plumbaginous substance formed upon it does not impede its electrical action. The President formerly anticipated the deposition of earthy substances upon the negative copper, and this he now found to take place upon sheets of copper exposed about four months to sea water, and defended by from $\frac{1}{30}$ to $\frac{1}{60}$ their surface of zinc and iron; they became coated with carbonate of lime and magnesia; but this effect is easily prevented, by duly diminishing the proportion of the protecting metal, so as to prevent the excess of negative power in the copper which then remains bright and clean.

The author observed that many singular facts had occurred in the course of his researches, some of which bore upon general science. Weak solutions of salt act strongly upon copper, but strong ones do not affect it, apparently because they contain little air, the oxygen of which seems necessary to give the electro-positive power to these menstrua. Upon the same principle, alkaline solutions and lime-water prevent the action of sea-water on copper, having in themselves the positive electrical energy which renders the copper negative.

The President concluded this paper with some further applications of electro-chemical theory to the subject of it, and referred to the principles developed, as suggesting means of preserving instruments of brass and of steel, by iron and by zinc, a circumstance already taken advantage of by Mr. Pepys, in enclosing delicate cutting instruments in handles or cases lined with zinc.

The Society then adjourned for the long vacation.

Art. XI. *Proceedings of the Royal Institution*, 1824.

The Lectures were commenced in the Amphitheatre of this Institution on Saturday, the 7th of February, when an introductory discourse was delivered by Mr. Brande.

The following arrangements in respect to the Lectures were announced to the Members and Subscribers.

On Electricity, Electro-Chemistry, and Electro-Magnetism. By William Thomas Brande, Esq., F.R.S. London and Edinburgh, Professor of Chemistry to the Royal Institution. This Course of Lectures will comprise an experimental Illustration of the Elementary doctrines of Electricity bearing upon its applications to Chemical Science and to the Theory and Phenomena of Magnetism. To commence on Saturday the 7th of February, at Two o'Clock, and to be regularly continued on each succeeding Saturday, at the same hour, till further notice.

On the leading Subjects of Mechanical Philosophy, and their recent Improvements, particularly Optics and Hydraulics. By John Millington, Esq., F.L.S., Sec. Astron. Society, *&c.*, Professor of Mechanics to the Royal Institution. To commence on Thursday the 12th of February, at Two o'Clock, and to be regularly continued on each succeeding Thursday, at the same hour, till further notice.

On Botany, with the Principles of Vegetable Physiology. By John Frost, Esq., Professor of Botany to the Medico-Botanical Society of London. To commence after Easter.

On Plane Geometry. By John Walker, Esq., formerly Fellow of Trinity College, Dublin, and M.R.I.A. To commence after Easter.

On Music. By W. Crotch, Mus. D., Professor of Music in the University of Oxford. To commence after Easter.

On Zoology, comprehending a Survey of the Class Mammalia. By J. Harwood, M.D., F.L.S.

On Europæan Literature. By the Marquis Spineto.

On Genealogy. By   Banks, Esq.

On the Objects of Vegetable Chemistry, and the applications of

Chemical Science to the elucidation of Vegetable Physiology. By W. T. Brande, Esq., F.R.S., and Prof. Chem. R.I.

The following are such of the Prospectuses of these Lectures, as have been published;

Prospectus of Mr. Brande's Lectures on Electricity.

Lecture I. *Saturday, February 7.*

History of Electricity, with experimental Illustrations. Account of the electrical and magnetical Discoveries of Dr. Gilbert. Researches of Wall, Hauksbee, Grey, Wheeler, and Watson. Theories of Electricity. Galvani's Experiments. Volta's Inquiries. Sketch of Sir H. Davy's Discoveries in relation to this subject, and of their influence upon the progress of electrical and chemical Science. Œrsted discovers the electrical production of Magnetism.

Lecture II. *February* 14.

An inquiry into the present state of the Theory of Electricity. Imperfection of all the hypotheses. Of the validity of Coulomb's Deductions. Phenomena of attraction and repulsion exhibited and explained in reference to the hypotheses of Du Fay, and of Franklin. Of Electroscopes and Electrometers. Investigations of Æpinus. Of Conductors and Nonconductors. History of the Electrical Machine—its various constructions, and their respective advantages. Analogies between the phenomena of Electrical and of Magnetic Attraction.

Lecture III. *February* 21.

Of the luminous appearances connected with Electrical Excitation. Electric Spark in various media. Electrified Points. Of induced Electricity, and of the Phenomena exhibited by Conductors of various kinds when under the influence of electrical induction. Of the Electro-Polar State. Of the causes of the accumulation and discharge of Electricity. Extensive Induction exhibited in the spiral tube and luminous words. Induction through air, glass, mica, lac, other media.

Lecture IV. *February* 28.

On the construction and theory of the Leyden Phial. Dr. Frank-

lin's Views. Polarity of a series of Jars. Electrometers applicable to measuring the intensity of the charge of a Jar. Experiments in reference to the Theory of the Leyden Jar. Magnetic Phenomena analagous to those of induced electricity. Of the Electrophorus, and the permanent source of its electricity—states of the upper and lower plates. Of Electrical Batteries.

Lecture V. *March* 6.

Excitation of Magnetism observed in wires transmitting electricity; independent effects of quantity and intensity. Experiments on the perforation, disruption, and ignition of various substances by Electricity. Experiments of Cavendish, Priestley, Bennet, and Volta, in relation to the Chemical agencies of Electricity. Dr. Wollaston's Researches upon this subject. Natural phenomena dependent upon or connected with Electrical Excitation. Applications of Conductors to houses, steeples, ships, and powder magazines.

Lecture VI. *March* 13.

Recapitulation of the principal subjects discussed in the preceding Lectures, as illustrating other sources of Electricity, and especially that of the contact of dissimilar metallic and other conducting bodies. Experiments of Galvani—of Volta. Construction of the Voltaic Pile—the *Couronne des Tasses*—the Battery. De Luc's Electrical Column—its importance in demonstrating the source of electricity in Volta's Pile. Influence of chemical agents in these arrangements. Sir H. Davy's early Discoveries in this department of science. Influence of the size of the plates, and of their number upon the electric excitement.

Lecture VII. *March* 20.

Proofs of the identity of Voltaic and common Electricity. Various forms and constructions of the Pile and Battery. Best construction suggested by Dr. Wollaston. Experiments on the ignition and fusion of substances with large Voltaic Batteries. Various causes which influence the conducting powers of Metallic Wires referred to in illustration of Sir H. Davy's Researches. Passage of Voltaic Electricity through a vacuum.

Lecture VIII. *March 27.*

Hypothetical Views in reference to the relation subsisting between heat, light, electricity, and magnetism. Usually received Theories of light and heat—their insufficiency. Of the Calorimotor and Magnetomotor. Electricity considered as a chemical agent. Early experiments of Nicholson, Carlisle, and Cruickshank. Series of experiments in illustration of Sir H. Davy's discoveries, commencing with those respecting the source of acid and alkaline matter in water, and terminating with the discovery of the nature of the Earths, fixed alkalies, and other substances. His application of a negative power to the prevention of the corrosion of the copper sheathing of ships.

Lecture IX. *April 3.*

Of the Electro-magnetic Discoveries of M. Œrsted. Positions assumed by magnets is respect to wires transmitting electrical currents in different directions. Dr. Wollaston's hypothesis of the cause of these phenomena explained by experiments and models. All *metals* susceptible of magnetism, but no other substances. Effect of Spirals. M. Ampere's experiments and apparatus. Modes of conferring permanent magnetism upon steel bars. Mr. Faraday's inquiries connected with this subject—first effects electro-magnetic rotation—his apparatus. Various means of exhibiting electro-magnetic rotation. Conclusion of the Course.

Syllabus of a Course of Lectures on Botany.

By Mr. Frost.

Lecture I. *Wednesday, May 26.*

Introductory Remarks on Vegetable Physiology. The Analogy between Plants and Animals considered. Definition of a Plant. Observations on the Textures and Vessels of which it is constituted. Examination of the component parts of the Trunk, *viz.*, the Epidermis, the Cortex, the Liber, the Alburnum, the Wood, and the Medulla.

Lecture II. *Wednesday, June 2.*

Consideration of the Sap and the Juices of Plants. The effects of

Light and Heat on Vegetables. Remarks on the Germination of Seeds, with the progress of the Corculum or Embryo. Use of the Cotyledons.

Lecture III. *Wednesday, June 9.*

Examples of Ascidia, or hollow-foliaceous Appendages. Remarks on the various kinds of Stems, *e. g.*, the Scape, Peduncle, Culm, &c. Illustrations of the Linnæan System.

Lecture IV. *Wednesday, June 16.*

Further Examples of the Artificial System. Remarks on the Natural Arrangement of Plants. Conclusion of the Course.

SYLLABUS OF MR. WALKER'S LECTURES ON PLANE GEOMETRY.

☞ The numeral references are to the Propositions in the Elements of Euclid.

Preliminary Remarks and Definitions.

Lecture I. § i. *Monday, May 10.*

Elementary Doctrine of Angles.

Prop. 13, 14, 15, El. Remarks on the Doctrine of Parallels. Prop. 29, 28, 27, El. Corresponding subtenses of Angles a test of their equality or inequality. Prop. 4, (first part) 24, 25, El.

Lecture II. § ii. *Monday, May 17.*

Elementary Doctrine of Triangles.

Prop. 32, El. and Corollaries. Three cases of identical Triangles. Prop. 4, 8, 26, El. Prop. 5, 6, 18, 19, El. Prop. 20, El. The line drawn from the right angle of a right-angled triangle to the middle point of the hypotenuse is equal to half the hypotenuse.

Lecture III. § iii. *Monday, May 24.*

Elementary Doctrine of Parallelograms.

Prop. 33, 34, 35, 36, 37, 38, 39, 40, 41, El. Converse of Prop. 34. Prop. 43, El. A parallelogram is bisected by any line drawn through the middle point of its diagonal. Prop. 2, 3, 4, 7, El. b. ii.

Lecture IV. § iv. *Monday, May 31.*

Doctrine of Triangles continued.

Prop. 2. El. b. vi. as far as it extends to commensurable lines. The three lines drawn from the angles of a triangle to the middle points of the opposite sides have a common point of intersection. Also, the three lines bisecting the angles of a triangle. Prop. 47, 48. El. Pappus's Theorem. Prop. 12, 13. El. b. ii.

Monday, June 14. § v. Problems.

SYLLABUS OF A COURSE OF LECTURES ON ZOOLOGY, COMPREHENDING A SURVEY OF THE CLASS MAMMALIA. BY J. HARWOOD, M.D., F.L.S., &c.

Lecture I. *Wednesday, April* 28.

Introductory Observations. Necessity of systematic arrangement in the study of Animated Beings. The bony fabric in Quadrupeds. Division of the Mammalia.

Order 1st. The Ape tribes, or Quadrumana; their curious adaptation in form and structure to their natural habits and economy. The Ourang Outang. Peculiarities in form, character and manners in African Monkeys. Baboons, American species, Lemurs. Galago, &c.

Order 2d. The Beasts of Prey or Carnivora; their diversity in form and habits; perfection of their senses; relative powers of destruction; modes of attack. The Bats. Other insectivorous Quadrupeds.

Lecture II. *Wednesday, May* 5.

Carnivorous Animals continued. Bears. Dogs. Hyenas. Feline tribes. Geographical distribution of species. Amphibious Quadrupeds. Seals. Walruses, &c. Extinct fossil species of the Carnivora. Caves in Germany containing them. Manner in which their remains are deposited. Kirkdale Cave.

Order 3d. The Gnawing Animals, or Rodentia. Peculiarities in the formation and habits of the principal genera.

Lecture III. *Wednesday, May* 12.

Order 4th. The Edentata. Remarkable formations and economy in the Sloths, Anteatus, Armadillos. Gigantic fossil species resembling the Sloth.

Order 5th. The Pachydermata. Beautiful and advantageous structure of teeth in vegetable feeders. Living Elephants; their formation and natural manners. Lost species, or Mammoth. Mastodons, Rhinoceros, Hippopotamus, all former inhabitants of Europe. Newly discovered living Tapir. The Stag genus. The Horse.

Lecture IV. *Wednesday, May* 19.

Order 5th. The Cattle, or Ruminantia. Rumination; other kind provisions in their favour. Wild and domesticated species of Cattle. Camels. Deer. Antelopes. Giraffe. The Sheep, Goat, and Ox genera. Gigantic fossil species of Deer and Ox.

Order 6th. The Cetacea or Whale tribes; their structure, natural manners, and relation to the rest of the Class. Conclusion of the Course.

Syllabus of a Course of Lectures on Music. By Dr. Crotch.

Lecture I. *Friday, May* 7.

Remarks on the National Music of various Countries—Definition. National Music supposed to be derived from the Music of the Ancients. On traditional Accuracy. Remains of the Music of the Greeks. Jewish Chants. The National Music of Ireland.

Lecture II. *Friday, May* 14.

The National Music of Scotland—Highland and Lowland. The National Music of Wales.

Lecture III. *Friday, May* 21.

National Music supposed to be English;—that of France, Italy, Switzerland, Germany, Spain and Portugal, Hungary, Poland, Scandinavia and Norway, Denmark, Russia, Sclavonia, Turkey, Arabia, Persia, the East Indies, China, Java, Otaheite, Canada, and Norfolk Sound.

Lecture IV. *Friday, May* 28.

Superiority of Vocal over Instrumental Music. Remarks on Mozart's Comic Opera of Cosi fan tutte.

Lecture V. *Friday, June 4,*

On the Progress of Improvement in the Opera. Remarks on Cosi fan Tutte continued.

Lecture VI. *Friday, June 11.*

Remarks on the Opera of Cosi fan tutte concluded. Character of Mozart.

Prospectus of Mr. Brande's Lectures on Vegetable Chemistry.

Lecture I. *Saturday, May 8.*

Objects of this department of Chemical Science—Of the structure and growth of Seeds—Influence of air and water upon Vegetation—Of heat and light—Structure of the root, trunk, branches and leaves—peculiar functions of the latter—Their influence upon the constitution of the Atmosphere. Growth of aquatic plants. Relative effects of different plants upon the soil in which they vegetate. Of the sap of plants and the theories of its circulation.

Lecture II. *Saturday, May 15.*

Methods of Chemical Analysis applicable to organic products—Destructive distillation—Methods of purifying the vinegar furnished by this process.—Production of coal gas.—Ultimate composition of several vegetable substances—How far consistent with the theory of Proportionals?—Use of chlorine in these analyses. Means of discovering the immediate or proximate principles of vegetables by the action of solvents and tests—Properties of starch—gluten—gum—sugar. Mutual conversions of these substances into each other—Relative nutritive powers of different vegetable substances used in food.

Lecture III. *Saturday, May 23.*

Chemical history of the proximate principles of vegetables continued—Of the different kinds of oil—Economy of oil gas illumination. Manufacture of soap—Volatile oils—resins—guaiacum—gum-resins—balsams—Of the astringent principle in vegetables, and its appli-

cations in the art of Tanning—Compositions of writing and printing ink—Of the colouring matter of vegetables—Of colour-making—Dyeing—Methods of conferring permanence upon vegetable colours—Outline of the art of calico-printing—Of the newly discovered salifiable bases—Cinchonia—Quinia—Morphia, &c.

Lecture IV. *Saturday, May* 29.

Of the theory of Fermentation and the production of beer and wine—Components of wort and of must.—Change of sugar, mucilage and starch into alcohol—Conversion of beer and wine into vinegar—Properties of acetic acid—Of the different quantities of alcohol contained in various fermented liquors, and of the state in which it exists in them—Of the properties and composition of pure alcohol—Action of acids upon alcohol—Preparation, properties, and composition of sulphuric ether—General views connected with this department of Chemistry—Conclusion of the Course.

On Saturday the 1st of May, the General Annual Meeting for the election of a President, and the other Officers, was held at the House of the Institution, when

The Right Hon. Earl Spencer, K.G. F.R.S. F.AS., &c., was elected *President*.

Treasurer, Sir Scrope Bernard Morland, Bart., M.P. LL.D.
Secretary, John Guillemard, Esq., F.R.S.

MANAGERS.

Baker, Sir Frederick, Bart., F.R.S.
Chamier, John, Esq.
Daniell, Edmund Robert, Esq.
Davy, Sir H. Bart., LL.D. P.R.S. M.P.
Duckett, Sir George, Bart., F.R.S. and F.S.A. V.P.
Hallam, Henry, Esq.
Hatchett, Charles, Esq., F.R.S. and F.S.A. V.P.
Lansdowne, Marquis of, LL.D. and F.R.S. V.P.
Moore, Daniel, Esq., F.R.S. F.S.A. and F.L.S. V.P.

Russell, Jesse Watts, Esq., M.P. F.R.S.
Scott, Sir Claude, Bart., F.L.S. and F.H.S. V.P.
Snodgrass, Thomas, Esq. F.R.S. V.P.
Soane, John, Esq., F.R.S. and F.S.A.
Somerset, Duke of, F.R.S. V.P.
Staunton, Sir George Thomas, Bart., M.P. F.R.S.

VISITERS.

Ansley, Col. Benjamin, F.H.S.
Antrobus, Sir Edmund, Bart., F.R.S., and F.S.A.
Bostock, John. M.D. F.R.S. and F.L.S.
Chichester, Earl of, F.R.S.
Children, John George, F.R.S. and F.L.S.
Daniell, John Frederic, Esq., F.R.S.
Fuller, John, Esq.
Leake, Lieut.-Col. W.M. F.R.S.
Leigh, James Heath, Esq.
Robinson, Rev. Sir John, Bart.
Solly, Richard Horsman, Esq., F.R.S. and F.S.A.
Sotheby, William, Esq., F.R.S. and F.S.A.
Stanley, Sir John Thomas, Bart., F.R.S. and F.S.A.
Taylor, George Watson, Esq., M.P.
Weyland, John, Jun. Esq. F.R.S.

At this meeting the Professor of Chemistry made a report of the Proceedings in the Laboratory of the Royal Instutution, in which he enumerated the various investigations that had been carried on there, and took a general view of the progress of Chemical Philosophy during the preceding year. He then adverted particularly to the new features that had been given to the Science by the discoveries of Sir H. Davy, and to the high reputation which the Royal Institution had thence derived; he trusted that it still contributed more towards the promotion and extension of chemical Science than any similar establishment, similarly endowed, and that its real and permanent objects were preserved and promoted, more especially in the extended course of practical lectures delivered in the Laboratory; in which, he observed, an attempt is made to set forth with due diligence and dignity those new principles in chemical philosophy which have exclusively emanated from this establishment, to point out their originality and im-

portance, and to preserve unsullied by jealousy, and unbiassed by prejudice, a School of Chemistry, which it is hoped may not be altogether unworthy the name of its founder.

The following are the Terms of Admission into the Royal Institution.

Members, An Admission-fee of Five Guineas, and a Bond for the annual payment of Five Guineas; or, in lieu of Admission-fee and Annual Payments, £57 15*s*.

Members, being Life Subscribers, who have compounded, or been nominated under the Act of Parliament, an Admission-fee of Three Guineas, and a Bond for the annual payment of Three Guineas; or, in lieu of Admission-fee and Annual Payments, £34 13*s*.

Members, on Admission, are to pay a Sum not less than Five Guineas, to be applied to the maintenance either of the Library or of the Mineralogical Collection, or of the Mechanical Collection or Model Room, at the option of the Member.

Members, for every additional Subscription of Twenty Guineas at one time, or of Three Guineas per annum, are entitled to introduce personally, or by a written order signed by themselves, one Visiter to each of the public Lectures, the Name and Residence of such Visiter being specified on the order of admission; (exclusive of the privilege reserved by Act of Parliament to those persons who were heretofore Proprietors.)

Members elected previously to the 4th of August 1823, who shall contribute annually *One Guinea* to the *Laboratory Fund*, or Ten Guineas at once, shall (exclusive of the privilege reserved by Act of Parliament to those who were heretofore Proprietors) have the power to admit one person to the Collection, Lectures, and Reading-Rooms, whenever they shall personally attend, but not otherwise.

Members contributing to the Laboratory Fund the Sum of One Hundred Guineas at one time, shall be Patrons for Life of the Laboratory, and shall have the power to appoint any other person, being a Member of the Institution, to be a Patron for Life of the Laboratory, and each of such Patrons may admit one person to all Chemical Lectures delivered in the Institution.

Members who shall contribute to the Library, or to the Collection of Minerals, or to both, or to the Mechanical Collection and Model Room, to the amount of One Hundred Pounds, or upwards, shall be Patrons for

Life of the Library and Mineralogical Collection, Mechanical Collection, and Model Room, and shall have the power to appoint any other person being a Member of the Institution) to be a Patron for Life, and each of such Patrons may admit one person daily to the Library and Mineralogical Collection, Mechanical Collection, and Model Room.

Annual Subscribers admitted before the 1st of January, 1825, £4 4*s*., and One Guinea to the Library Fund on their Admission.

Annual Subscribers admitted after the 1st of January, 1825, £5 5*s*., and One Guinea to the Library Fund on their Admission.

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*** *Members* have admission to all the public parts of the Institution, and to all the Lectures, including those delivered in the Laboratory.

Annual and Life Subscribers have Admission to the Library, Reading, and Newspaper Room, and to all the Public Lectures, and are likewise admitted to Mr. Brande's Chemical Lectures delivered in the Laboratory on payment of an additional Sum of Two Guineas for each Course, or Six Guineas for an unlimited attendance, but are not admitted to any of the General or other Meetings of the Members.

The Annual Report *of the Visitors of the* Royal Institution *of Great Britain.*

Albemarle-street, 12th April, 1824.

In presenting the usual Abstract of Accounts for the year 1823, the Visiters of the Royal Institution are happy to have it in their power to announce to the members the commencement of a new era.

It will be recollected that, in their last report, they felt it to be their duty strongly to represent the propriety of inquiring into the state and prospects of the establishment, and the paramount necessity of equalizing the income and expenditure, both by judicious retrenchment and an appeal to the liberality of the members. The managers did not hesitate immediately to adopt the suggestion which was thrown out, of summoning a general meeting, by whom a committee was appointed, who, with much labour and perseverance, went through the necessary investigation, and drew up a plan to meet the exigencies of the case. The report of this committee has been already sent to the members, and it is only neces-

sary to remark that most of the suggestions contained in it have been carried into effect by the Board of Managers.

By the liberality of the president and a few members (to one of whom most particularly the Institution is indebted for the suggestion, and a munificent contribution towards carrying it into effect) a loan, without interest, has been raised, to pay off all the outstanding demands; so that the ruinous expedient of borrowing money at an interest of five per cent. to meet the current expenses, will no longer be deemed necessary.

The laboratory has been placed upon a new foundation, and the expense of its maintenance has been withdrawn from the general fund. It will in future depend for its support upon the liberality of the old members, and an additional contribution from the new. There can be no doubt that these means will be amply sufficient to the end in view; for when it is considered that the question to be determined is, whether or not the Royal Institution shall maintain the high consideration it enjoys by means of original investigation and experiment, which of the members will refuse his contribution? One guinea per annum from each member, it has been calculated, will be sufficient to maintain the laboratory in its present state of efficiency, and for this small sum the privilege of personally introducing one person to all the public lectures has been conferred. The list of donations and contributions to the laboratory fund, which accompanies this report, will prove that the subscription has been begun with alacrity and spirit.

The admission fees and compositions of members will no longer be considered, as heretofore, in the light of annual income, but will be immediately vested in public securities; and, after paying off the loan, will be carried to the account of the permanent fund, which some years ago was so liberally begun by one of the members.

With regard to the expenditure of the last year it may be necessary to make one remark as, without some explanation, the total may be deemed excessive. Amongst the items is included a sum of 600*l.* which was paid to the city of London, as a fine for the renewal of the lease of the premises. By their under-leases

the whole of this amount, and 10*l.* in addition, should have been received from the tenants of the Institution, but only 370*l.* has actually been recovered, one of the tenants being unable to fulfil his covenant, whereby his right of renewal has become forfeited, and is now vested in the Institution; the balance of 240*l.* may therefore be considered as a sum of money laid out to great advantage.

It is not the duty of the visiters to anticipate the accounts of the current year; but, having taken some pains to ascertain the scale of the present expenditure, they cannot resist the gratification of communicating the result of their inquiries, which has been highly satisfactory.

The estimated diminution of expense (excluding the 600*l.* fine) is about 600*l.*, to which must be added 300*l.* for the charge of the laboratory, making a reduction of 900*l.* in the charge upon the general funds.

The privilege of introducing one person to the lectures, which has been given to the members who have subscribed to the laboratory fund, has been found not to interfere with the number of annual subscribers, which is, on the contrary, larger than in the preceding years; and the visiters have had the gratification to find that the lectures in all cases have been well attended, and that the theatre on some occasions has been full.

Under all these favourable circumstances, the visiters cannot refrain from congratulating the members upon the new aspect which the affairs of the Royal Institution have assumed. They do not hesitate to give it as their opinion, that the whole establishment is now placed upon a much securer foundation than that of any former period; and if they feel any anxiety, it is that the members, by their patronage of the laboratory fund, may not only enable that department to persevere in those exertions, from which such great benefits have already been derived to science, and such honour to the nation, but may increase its efficiency, and enlarge the sphere of its utility.

WINCHILSEA,	BENJAMIN ANSLEY,
JOHN FULLER,	JOHN ROBINSON,
R. H. SOLLY,	J. F. DANIELL.

Art. XII. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. XVIII.

i. *Extracts relating to the Theory of the* Tides.

[It is thought advisable to insert, in this article, a popular view of the principal phenomena of the tides, which appeared some years ago, in a periodical work of a miscellaneous nature, and to add to it some few corrections, and some extensions, which form a part of a more scientific investigation, that has lately been published in the Supplement of the *Encyclopædia Britannica.*]

" Of the objections," says a writer in the *Quarterly Review* for October, 1811, p. 76, [which have been hastily advanced by some authors, against the established theory of the tides,] " the most material seems to be, that according to the Newtonian opinion, the moon must be supposed to repel the waters on the remoter side of the earth, instead of attracting them. The next is, that the lunar action must be sufficient to overcome the forces of gravity and cohesion. The third, that the time of high water is frequently three, and sometimes six hours later, than that of the moon's passage over the meridian.

" The difficulty in conceiving the apparent repulsion of the waters on the remoter side of the earth, which very naturally occurs to one who is but little conversant with the subject, appears to depend on a want of sufficient attention to the manner in which the mean solar and lunar attractions are counterbalanced. We are unconsciously disposed to consider the earth, especially in comparison with the moon, as a body perfectly at rest, or at most as an immense sphere poised on its axis, or having some secret support connected with its centre. And it is true that, if the earth were suspended as an apple hangs on a stalk, or a terrestrial globe on the pins which connect it with the brazen meridian, the attractive force of a distant body would necessarily tend to collect a fluid surrounding it, about the part nearest to the disturbing body.

But in fact the force counteracting the solar and lunar attraction is by no means to be confounded with the operation of a support of any kind, attached to the solid parts of the sphere alone; for the force actually concerned in this case is equally efficacious with respect to the fluid parts; and, acting exactly alike on every particle of the earth and sea, it precisely counterbalances the mean force of attraction, and leaves only the difference of the attractive powers, which are different for the different parts of the earth and sea, to exhibit its effects in disturbing the relative situations of those parts. This counterbalancing power is well known under the name of the centrifugal force, being derived from the velocity of the earth, either in its annual revolution round the sun, or, in the case of the moon, from its velocity in revolving round the common centre of inertia of the earth and moon. Since the earth actually falls at every instant as much within the tangent of its annual orbit, or the temporary line of direction of its motion, as it would descend towards the sun in an equal time, if it were otherwise at rest, this change of relation of the revolving body, which prevents its actual approach to the centre of attraction, and counteracts the force of gravitation, is, not improperly, considered as constituting a distinct force, and is characterized by the term centrifugal. Before the introduction of the Newtonian theory, an attempt was made, by the celebrated Dr. Wallis, to deduce the tides from a difference of the centrifugal forces affecting the opposite parts of the earth and sea, in revolving round the sun, and round the common centre of gravity of the earth and moon; and Mr. Ferguson, in later times, has endeavoured to explain an opinion of a similar nature, by means of the whirling table; but the apparatus of Ferguson was so constructed, as to produce a greater velocity of rotation in the remoter than in the nearer parts of the revolving system of bodies, which is a difference that does not exist in the case to be investigated; for the velocity of the different parts of the earth and sea, with respect to their common annual revolution round the sun, is precisely the same, the diurnal rotation being altogether independent of this revolution, and producing modifications of force, which have their separate compensations, as distinctly indeed as

the monthly revolution of the moon, which does not affect the velocity of its mean annual revolution round the sun, together with the earth.

"It is therefore so far from being true, that the inequality of the centrifugal force, at different parts, gives rise to any part of the phenomena of the tides, that, on the contrary, the perfect uniformity of this force is the basis of the determination of the powers immediately concerned in these phenomena. The Atlantic and the Pacific oceans are subjected to a centrifugal force precisely equal to that which affects the solid parts of the earth; but when the luminary is over the Atlantic, its attraction for that ocean is greater than for the central part, and consequently greater than the centrifugal force, so that this differential attraction tends to elevate the Atlantic; at the same time that its attraction for the Pacific ocean is less than the mean attraction, and less than the centrifugal force, which therefore prevails over the attraction, and the differential force tends to raise the Pacific ocean almost as much as it tends to raise the Atlantic in the opposite direction.

"There is also an additional force, derived from the obliquity of the action of the luminary on the parts of the earth not immediately below it, which tends to compress the lateral parts, and to increase the elevation at the ends of the diameter pointing to the luminary.

"The readiest way of calculating the operation of all these forces is, to reduce them to a horizontal direction, and to determine what inclination of each part of the surface of the sea, considered as an inclined plane, will cause such a tendency, in a particle situated on it, to move in a contrary direction, as precisely to counterbalance, not only these forces, but also the new disturbing force, derived from the attraction of the parts thus elevated; and it may easily be shown, that all these conditions will be fulfilled, if we attribute to the surface of the sea the form of an oblong elliptic spheroid, differing but little from a sphere."

Now, "we have only to recollect, with respect to the first objection" already mentioned, "that we are by no means required to imagine that the moon repels the remoter parts of the earth and

sea; but merely to understand, that these parts are left a little behind, while the central parts fall more within the tangent, towards the moon, and the nearer parts still more than the central parts: nor is this a fact of which our belief must rest on any observed phenomena of the tides, since it is completely demonstrable from the general laws of gravitation and of central forces; so that if no such tides were under any circumstances observable, their non-existence would afford an unanswerable argument against the universality and accuracy of these laws, as they are inferred from other phenomena.

"The second objection is already answered in the statement of the mode of operation of the disturbing force. The action of this force is only supposed to be sufficient to retain a particle of water in equilibrium on a surface of which the inclination to the horizon is scarcely perceptible, or to cause the whole gravitation of a column four thousand miles in height, immediately under the luminary, to be equal only to the gravitation of a column shorter by a few inches or feet, in another part of the spheroid. The objectors have confounded this very slight modification of the force of gravitation, with its complete annihilation by a greater force: and with respect to the force of cohesion, it is so little concerned in counteracting any elevation of this kind, that to attempt to calculate the magnitude of any resistance derived from it would be perfectly ridiculous.

"The third objection is only so far more valid, as it is opposed to the imperfect and superficial notions," which some authors have entertained, "of the supposed operation of the forces concerned:" as if the sea could instantly accommodate itself to the temporary form which would afford an equilibrium. In fact, however, it is just as likely to happen, in the open ocean, that the transit of the luminary may coincide with the time of low water as with that of high water; and in more limited seas and lakes, there is no hour in the twenty-four at which high water may not naturally be expected to take place, according to the different breadth and depth of the waters concerned; while, under other circumstances, it may happen to be high water only once a day, or once a fortnight, or there may be no tide at all, without any deviation from the strictest

regularity in the operation of the causes concerned. Since the subject has hitherto been considered as extremely intricate, and has not indeed yet been freed from all its embarrassments, we shall here endeavour to explain the principles on which the investigation has been conducted.

"The attempts that were made by Newton, to compute the effects of the solar and lunar attraction on the sea, went no further than to the determination of the magnitude of the elevation which would at any given time afford a temporary equilibrium: and even Maclaurin was satisfied with having ascertained the precise nature of the form which the waters must assume in such a case. But it is obvious that these determinations are by no means sufficient for ascertaining the motions which arise from the change of relative situation induced by the earth's rotation, since the form, thus ascertained, only affords us a measure of the force by which the waters are urged, when they do not accord with it, and by no means enables us to say, without further calculation, how nearly they will at any time approach to it. In fact, the change of the conditions of equilibrium determines only the magnitude of this force, such as it would be if the sea remained at rest, while it is in reality materially modified, at any given time, by the effect of the motions which have previously taken place: and supposing its true magnitude to be ascertained, its immediate operation will at all times be complicated with the conditions, under which an impulse of any kind is capable of being communicated to the neighbouring parts of the sea, which depend on the depth of the sea, as well as on the form of the earth.

"Mr. Laplace has undertaken the investigation of the theory of the tides, with all these additional complications; and he considers it as constituting, without exception, the most difficult department of the whole science of astronomy; and yet this consummate mathematician has omitted to include in his calculation the effects to be attributed to resistances of various kinds, and to the irregularities of the form of the sea, which appear to us to constitute by far the more material difficulties in the inquiry. The general problem, relating to the oscillation of a fluid completely covering a sphere, and moving with little or no resistance, which Mr. Laplace

has solved by a very intricate analysis, is capable of being exhibited in a much less embarrassed, and, we apprehend, even in a more accurate manner, by a mode of investigation, which is equally applicable to the tides of narrow seas and of lakes, and which may easily be made to afford a correct determination of the effects of resistance, as well as a ready mode of discovering the laws of motions governed by periodical forces of any kind; at least so far as these forces are capable of being represented by any combinations of the sines of arcs, which increase uniformly with the time.

"The essential character of this method consists in comparing the body actuated by the given force to a pendulum, of which the point of suspension is caused to vibrate regularly to a certain small extent: the length of the pendulum being supposed to be such as to afford vibrations of equal frequency with the spontaneous vibrations of the moveable body, and the point of suspension to be carried by a rod of such a length, as to afford vibrations of equal frequency with the periodical alternations of the force. It is then shown, that such a pendulum may perform regular vibrations, contemporary with the alternations of the periodical force, and inversely proportional in their extent to the difference between the length of the two rods: and that, whatever may have been the initial state of the pendulum, the motion thus determined may be considered as affording a mean place, about which it will at first perform simple and regular oscillations; but that a very small resistance will ultimately cause these to disappear:" so that the particular solution of the problem, which indicates a series of vibrations as they *may* be performed, is thus rendered general; since every other initial state of the vibrations *must* ultimately terminate in this series.

"Now the sea, or any of its portions, may be considered as bodies susceptible of spontaneous vibrations, precisely similar to the small vibrations of a pendulum; and the semidiurnal variation of the form which would afford an equilibrium, in consequence of the solar and lunar attractions, is perfectly analogous to the regular vibration attributed to the point of suspension of the pendulum. The frequency of the simple oscillations of the sea, or of any of its parts,

supposing their depth and extent known," and their form sufficiently simple, "may easily be deduced from the important theorem of Lagrange, by which the velocity of a wave of any kind," when sufficiently broad, "is shown to be equal to the velocity of a heavy body, which has fallen through half the height of the fluid concerned: but in the case of a tide extending to any considerable portion of the surface of the globe, this velocity must be somewhat modified according to the comparative density of the central and the superficial parts.

"The most remarkable consequence of this analogy is the law, that if the simple oscillations, of which the moving body is susceptible, be more frequent than the period of the recurring force, the pendulum will follow its point of suspension with a direct motion; but if the spontaneous vibrations be the slower, the motions will be inverted with respect to each other: and, with regard to the tides, we may infer from this mode of calculation, that supposing the earth to be between five and six times as dense as the sea, the oscillations of an open ocean can only be direct, if its depth in the neighbourhood of the equator be greater than fifteen or sixteen miles: and that if the depth be smaller than this, the tides must be inverted, the time of low water corresponding, in this case, to the transit of the luminary over the meridian.

"This distinction has not been explicitly made by Mr. Laplace, although he has calculated, that for a certain depth, of a few miles only, the tides of the open ocean must be inverted, and that for greater depths they will be direct: but the intricacy of his formulæ seems to render their use laborious, and perhaps liable to some inaccuracy; and in the application of his theory, he seems to have lost sight even of the possibility of an inverted tide. In narrower seas, which Mr. Laplace has not considered, a smaller depth will constitute the limit between these two species of tides; and in either case the approach of the depth to this limit will be favourable to the magnitude of the tide." It may also be remarked, that if the depth of the sea became gradually smaller in receding from the equator, till it vanished at the poles, its surface, as well as that of the earth, having the form of an oblate spheroid, the time re-

quired for a wave to travel round it would be equal in all latitudes, and the tides would be of the same species in every part; while the tides of the atmosphere, on the other hand, independently of the resistance, would be indirect at the equator, and direct near the poles.

"However the primitive oscillation may be constituted, it is easy to understand, that it will be propagated through a limited channel, connected with the main ocean, in a longer or shorter time, according to the length and depth of the channel; and that if the channel be open at both ends, the tide will arrive at any part within it by two different paths; and the effects of two successive tides may in this manner be so combined as to alter very materially the usual course of the phenomena: for instance, if there were about six hours' difference in the times occupied in the passage of the two tides over their respective paths, the time of the high water belonging to *one* tide would coincide with that of the low water belonging to the other, and the whole variation of the height might in this manner be destroyed, as Newton has long ago observed with respect to the port of Batsha: and it may be either for a similar reason, or from some other local peculiarity of situation, that no considerable tides are observed in the West Indies; if indeed it is true, that the tides are so much smaller there than might be expected from calculation; for in fact the original tides of an open sea, not exceeding a mile or two in depth, would amount to a few inches only, even without allowing for the effects of resistance. In the middle of a lake, or of a narrow sea, there can be little or no primitive elevation or depression; and the time of high water on its shores must always be about six hours before or after the passage of the luminary over the middle; so that from this source we may derive an infinite diversity in the times at which these vicissitudes occur in different parts.

"The effects of resistances of various kinds, in modifying the time of high water, cannot easily be determined in a direct and positive manner from immediate observation. Mr. Laplace appears to be of opinion that these resistances are wholly inconsiderable; but if any dependence can be placed on the calculations of

Dubuat, we ought to expect a very different result, since, according to Dubuat's formula, the resistance, in the case of a tide of any moderate magnitude, must far exceed the moving power. From this result, however, nothing can be concluded with certainty, except that the formula is extremely defective with respect to great depths and slow motions; yet we may infer from it, as a probable conjecture, that the resistance must be great enough to produce some perceptible effects, and even that it must be greater than would be expected from another mode of calculation founded on the same experiments, (*Phil. Trans.* 1808; *Suppl. Enc. Br.* Art. HYDRAULICS,) which would give the proportion of the greatest resistance to the greatest moving force only as ¼ of the height of the tide, increased by about ten feet, to the whole depth of the ocean concerned, at least on the supposition of a uniform depth and a smooth bottom, which indeed must be far from the truth; since he inequalities of the bottom of the sea must tend very greatly to increase the resistance, especially that part of it which varies as the square of the velocity.

"Now it has been demonstrated, (*Nich. Journ. Illustr. Cel. Mech. Suppl. E. Br.* Art. TIDES,) that a resistance, simply proportional to the velocity, would not disturb the perfect regularity of the oscillations concerned, and that it would only retard them when direct, and accelerate them when inverted, by the time corresponding" to a certain arc, of which the tangent is to the radius, as the velocity due to half the length of the pendulum synchronous with the periodical force is to the velocity at which the resistance becomes equal to the force of gravity, and as the length of the pendulum synchronous with the spontaneous oscillation to the difference of the lengths of these two pendulums conjointly. "Nor will the displacement produced by an equal mean resistance, varying as the square of the velocity, be materially different; the body or surface merely oscillating a little about its mean place, in consequence of the different distribution of the resistance.

"Here, then, we have another source of very great diversities in the times of the tides, according to the dimensions of the seas concerned, even in those parts in which the tides may be supposed to

be rather original than derivative, not excepting the most widely extended oceans. There are, however, other considerations, which limit, in some measure, the probable magnitude of a resistance varying either accurately or very nearly in proportion to the square of the velocity; and the chief of these is the time of high water at the spring and neap tides, which must be very differently affected by such a resistance, since it must necessarily cause a much greater acceleration or retardation of the spring tides than of the neap tides. Hitherto it has only been observed that, in particular ports, the greatest tides have happened the earliest; but no accurate comparison of the times of high and low water have been made in a sufficient variety of circumstances to authorise our forming any general conclusion of this kind. It might indeed be supposed, that this diversity of the relative time of high water might be modified and concealed by a difference of velocity in the progress of the different tides from their source in the ocean to the places of observation, according to the different degrees of resistance opposed to them: but, if we can depend on a mode of calculation which has occurred to us, the velocity, with which a wave or tide is propagated, is not materially affected by a resistance of any kind, its magnitude only being gradually reduced, and even its form remaining little altered by this cause, when the resistance is nearly proportional to the velocity;" although, as the form of a wave is evidently altered in approaching the shore, its summit advancing more rapidly than its basis, till it falls over and the wave breaks, so a tide remote from the ocean is generally observed to rise somewhat more rapidly than it falls.

" Another limitation of the magnitude of a resistance, varying as the square of the velocity, is the modification of the apparent proportion of the solar to the lunar force, which must arise from it In assuming that the comparative magnitudes of the tides in the open sea must be precisely the same with those of the disturbing forces which occasion them, astronomers have hitherto neglected two very material circumstances; one, the effect that a greater approach of the frequency of the spontaneous oscillations, to the solar or lunar period, must have in augmenting the respective tide;

the other, the greater diminution of the spring than of the neap tides by the operation of a resistance proportional to the square of the velocity, which gives to the lunar tide a greater apparent preponderance. Mr. Laplace is obliged to have recourse to some imaginary peculiarities in the local situation of the port of Brest, in order to explain the existence of lunar and solar tides in the proportion of three to one, while the other phenomena, depending on the moon's attraction, make it improbable that the lunar force can be to the solar in a much greater ratio than that of five to two. But, in fact, the proportions of the tides in other ports, very differently situated, for instance at St. Helena, are nearly the same with those which have been observed at Brest; and it is demonstrable, that such a diminution of the apparent solar force must necessarily be the consequence of the operation of any resistance, proportional to the square of the velocity; besides being in part dependent, according to the most probable suppositions, upon the actual depth of the sea, as being more favourable to the exhibition of a lunar than a solar tide.

"There remains to be explained the interval which elapses between the time of new or full moon, and the occurrence of the highest tides, amounting at Brest to about a day and a half, and at London bridge probably to two days. The most simple supposition respecting this interval, is that which Mr. Laplace has adopted; as the retardation is greater at London bridge than at Brest, so it may be imagined that there are other places, still more exposed than Brest to the great oceans, at which it will altogether disappear. We cannot, however, discover any thing like a progressive succession of this kind in the tides which are observed at different parts of the continent; nor would so great a time as a day and a half be required for the passage of a tide over more than half the circumference of the globe, upon any probable estimate of the depth of the sea." The full development of the manner, in which the resistance may be supposed to cause this retardation, will be found in the Supplement of the Encyclopædia.

"We have assigned abundant reasons for the diversity which occurs in the time of high water at any given period of the moon's

revolution in places differently situated; and this time being once ascertained for any one tide, we may easily infer by calculation the time at which every other tide will occur; and we shall find in this sequence the most perfect coincidence between theory and observation. Thus, if the high water of the spring tides, derived from the coincidence of the solar and lunar high waters, soon after the new or full moon, happened at any port precisely at noon, the next time of the high water belonging to the solar tide would of course be at midnight, and that of the lunar high water twenty-five minutes later; and the true time of high water will divide this interval nearly in proportion to the apparent forces, and will occur about eighteen minutes after midnight," [the interval being 12^h 18^m, and not $12\frac{1}{2}^h$, as it has been hastily assumed for the table of the Supplement:] " and the next day it will be high water about thirty six minutes after twelve. This retardation will increase from day to day, since its mean daily value is about fifty minutes; and at the neap tides following the moon's quadratures, it will become about twice as great as at the syzygies, its different values, in these cases, being nearly proportional to the magnitude of the spring and neap tides; so that Bernoulli has considered them as affording the most correct estimate of the comparative magnitude of the solar and lunar forces; although they are probably less capable of being accurately determined by direct observation than the different elevations and depressions. We can scarcely imagine it possible that any individual, acquainted with these simple facts alone, to say nothing of many others, equally well established, could for a moment entertain the slightest doubt of the real and immediate dependence of the tides on a combination of the solar and lunar attractions."

" In the diurnal and annual variations of the height of the tides, there is no peculiar difficulty. The declinations and distances of the luminaries modify their forces in a manner which is easily determined; and the periods of these changes being much greater than the times of spontaneous oscillation in any of the seas concerned, the effects directly follow their causes, almost in the simple proportion of the intensity of the forces concerned. Mr. Laplace

has calculated, that in an ocean of equable depth, the difference between the heights of the morning and evening tides, depending on the declination of the luminary, must wholly disappear; but we cannot help suspecting that there must be an imperfection in some of the many steps of his investigation. The depth would be equable if the whole sphere were fluid; and it will not be denied that in this case there would be a difference in the morning and evening tides, very nearly coinciding with that of the primitive variations of the figure affording an equilibrium: nor can we discover any imperfection in the method, which Mr. Laplace himself has sometimes adopted, of considering the difference of the two tides as a separate diurnal tide, and determining its magnitude precisely in the same manner as if it existed alone."

"When a regular tide moves continually forwards in an open ocean, the progressive motion of the fluid is the greatest, or in other words, the flood is the strongest where the elevation is greatest, and the motion is retrograde, constituting the ebb, wherever there is a depression. In a river, the effect of a stream would only so far modify the velocity, as to make it proportional to the elevation above or the depression below a different level; but if a river or channel of any kind terminated abruptly, so as to cause a reflection, the progressive velocity would commence from the time of low water, and continue till that of high water only, or even be counteracted by the motion of the current, so as to cease still earlier, and to commence later. The rivers, in which our tides are commonly observed, seem to hold a middle place between these two cases: at Lambeth, for instance, the flow of the tide is continued, not during the whole time that the water remains elevated above a certain level, but about three quarters of an hour after the time of high water, at which it would cease near the end of a channel terminating abruptly. And it is probable that by similar considerations the course of the ebb and flood tides might be explained in many other cases."

"If we apply the same mode of calculation to the tides of the atmosphere, they will appear to be subject to some very singular modifications. At the poles they must be very small; at the equa-

tor moderate; but at the latitude of about 42°, where the rotatory velocity of the earth's surface is equal to the velocity with which any impression is transmitted by the atmosphere, or at about 40° of the lunar tide, the height of the oscillations will only be limited by the resistances, the greatest elevation occurring about three hours after the transit of the luminary; nearer the pole they will occur earlier than this, and nearer the equator a little later." Possibly, indeed, the slight obliquity in the direction of the high water might have some little tendency to equalize the height of the tides of different parts of the atmosphere: "it seems, however, to be a mistake to suppose, that the effects of the atmospherical tides must be more perceptible near the equator than in temperate climates; and the variations of the barometer, which have been observed between the tropics, are manifestly independent of the lunar attraction, occurring regularly at certain hours of the day or night; as indeed the tides of the ocean might have been expected to occur, if they had really been derived from the" meteorological causes to which some authors have "chosen to attribute them."

Of the article Tides in the *Supplement*, the first section relates to the "Progress of contemporary tides as inferred from the times of high water in different ports." The author's conclusions from a tabular comparison of observations are these:—

"First, that the line of contemporary tides is seldom in the exact direction of the meridian, as it is supposed to be universally in the theory of Newton and of Laplace; except, perhaps, the line of the twenty first hour [of Greenwich time] in the Indian ocean, which appears to extend from Socotora to the Almirantes, and the Isle of Bourbon, lying nearly in the same longitude.

"Secondly, that the southern extremity of the line advances as it passes the Cape of Good Hope, so that it turns up towards the Atlantic, which it enters obliquely, so as to arrive, nearly at the same moment, at the Island of Ascension, and at the Island of Martin Vaz, or of the Trinity.

"Thirdly, after several irregularities about the Cape Verd Islands, and in the West Indies, the line appears to run nearly east and west from St. Domingo to Cape Blanco, the tides proceeding due

northwards; and then, turning still more to the right, the line seems to become N.W. and S.E. till at last the tide runs almost due east, up the British Channel, [while another part of it passes] round the north of Scotland into the Northern Ocean, sending off a branch down the North Sea to meet the succeeding tide at the mouth of the Thames.

"Fourthly, towards Cape Horn again there is a good deal of irregularity; the hour-lines are much compressed between South Georgia and Terra del Fuego, perhaps on account of the shallower water about the Falkland Islands and South Shetland.

"In the fifth place, at the entrance of the Pacific Ocean, the tides seem to advance very rapidly to New Zealand and Easter Island; but here it appears to be uncertain whether the line of contemporary tides should be drawn nearly north and south from the Gallapagos to Terra del Fuego, or N.E. and S.W. from Easter Island to New Zealand; or whether both these partial directions are correct: but on each side of this line there are great irregularities, and many more observations are wanting before the progress of the tide can be traced, with any tolerable accuracy, among the multitudinous islands of the Pacific Ocean, where it might have been hoped that the phenomena would have been observed in their greatest simplicity, and in their most genuine form.

"Lastly, of the Indian Ocean the northern parts exhibit great irregularities, and among the rest they afford the singular phenomenon observed by Halley in the port of Tonkin, and explained by Newton in the *Principia:* the southern parts are only remarkable for having the hour lines of contemporary tides considerably crowded between New Holland and the Cape of Good Hope, as if the seas of those parts were shallower than elsewhere."

The second section relates to the "disturbing forces that occasion the tides," and presents nothing that is not readily demonstrable, and indeed universally admitted, except, perhaps, the magnitude of the primitive elevation, produced by the lunar and solar forces, which is made two feet and ten inches respectively, or at the very utmost 2¼ feet and eleven inches, for the actual density of the earth and sea, instead of the much greater height commonly

assigned to it, on the very erroneous supposition of a homogeneous sphere of water.

The third section investigates the "effects of resistance in vibratory motions, whether simple or compound," and reduces into a somewhat more technical or fashionable form the propositions which the author had before deduced from a geometrical mode of representation, but with considerable extensions and improvements: and as a corollary tending to illustrate the accuracy of his formulæ, he has applied them to the problem of a pendulum moving with a resistance proportional to the velocity, which had been left incomplete by Euler. He has shown that the resistance, in Captain Kater's experiments, could only have caused an error of a second in about fifty years: a quantity certainly altogether insignificant, but which could not with propriety be wholly neglected, while it was known that its magnitude was determinable, and while its insignificance remained undemonstrated.

He then proceeds to compute the effect of periodical forces with or without resistance, and shows that the effects of such forces on a pendulous or vibratory body are always most considerable when the period of the force approaches very near to that of the vibration: a proposition which is illustrated by the sympathetic vibrations of the pendulums of clocks, and in the motion of the inverted pendulum, invented by Mr. Hardy, as a test of the steadiness of a support, which shows, when it is well adjusted to the rate of a clock, that no pillar can be so steady as not to communicate to it a very perceptible motion by its regular, though extremely minute, and otherwise insensible change of place.

The theorem most immediately applicable to the case of the tides is this, (K): "the equation, $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs + M\sin. Gt = 0$, may be satisfied by taking $s = \frac{M}{\sqrt{([GG-B]^2 + AAGG)}}\sin.\left(Gt - \text{arcta}\frac{AG}{B-GG}\right)$." which is also extended by a subsidiary approximation to the case of a resistance varying as the square of the velocity.

In the fourth section of the article we find the "Astronomical determination of the periodical forces which act on the sea or on a lake," affording the equations which by means of Theorem K, could give

at once the height of the tides in any port, if the coefficients were sufficiently determined, and even without this determination affording some interesting conclusions from facts that are already well known.

For a canal or a sea lying in an easterly and westerly direction, the periodical force is shown to vary as sin cos *Alt.* sin *Az.*, and for a canal deviating from that direction in a given angle, as sin cos *Alt.* sin (*Az.* + *Dev.*). And in the two cases of a canal running east and west in any latitude, and of a canal situated obliquely at or near the equator, the force becomes, still more simply, first, L sin cos *Decl.* sin *Hor.* $\angle$ + L′ $\cos^2$ *Decl.* sin cos *Hor.* $\angle$, L being the sine of the latitude, and L′ its cosine; and secondly, if D be the sine of the deviation, or of the angle formed by the length of the canal with the equator, and D′ its cosine, D sin cos *Decl.* cos *Hor.* $\angle$ + D′ $\cos^2$ *Decl.* sin cos *Hor.* $\angle$.

A series is then found for representing the declination by means of arcs increasing uniformly with the time; but it is observed that for the purposes of calculation it is sufficient to suppose the sun and moon to move uniformly in the ecliptic, or even to have uniform motions in right ascension; whence we obtain for the sun's force, on a canal running east and west, putting $æ$ for the sine of the obliquity of the ecliptic, $\odot$ for the sun's longitude, and t for the horary angle, $S(\text{L}\,æ'\,[\frac{1}{2}\cos(t-\odot) - \frac{1}{2}\cos(t+\odot)] + \text{L}\,æ''\,[\frac{1}{2}\cos(t-3\odot) - \frac{1}{2}\cos(t+3\odot)] + \text{L}\,æ'''\,[\frac{1}{2}\cos(t-5\odot) - \frac{1}{2}\cos(t+5\odot)] + \frac{\text{L}'}{2}(-\frac{1}{2}æ^2)$ $\sin.\,2t + \frac{\text{L}'}{4}æ^2\,[\frac{1}{2}\sin 2(t+\odot) + \frac{1}{2}\sin 2(t-\odot)]$: $æ'$, $æ''$, and $æ'''$, being coefficients derived from $æ$, and equal respectively to about .3645, .0078, and .00002, and $æ^2 = .1585$. From each of the terms, expressing the forces, the value of the corresponding portion of the space described may be obtained by means of the general Theorem K, substituting, in the case of the solar tide, for the coefficient of the simple resistance A, the value $A' = A + 2.88\,DM'$, in which D is the coefficient of the resistance varying as the square of the velocity, and M' the supposed actual extent of the lunar tide; and for the lunar tide $A'' = A + 2.88\,DS' + .8484\,D(M' - S')$.

But without calculating the precise amount of all the coefficients, the author proceeds to demonstrate in general, that "the results, with regard to the space described, will not differ much from the proportion of the forces, except when their periods approach nearly to that of the spontaneous oscillation, represented by B." And "considering in this simple point of view the correct expression of the force; we may observe that the phenomena, for each luminary, will be arranged in two principal divisions, the most considerable being represented by $\frac{1}{2}$ (L′, D′) $\cos^2$ *Decl.* sin 2 *Hor.* $\angle$, and giving a tide every twelve hours, which varies in magnitude as the square of the cosine of the declination varies, increasing and diminishing twice a year, being also proportional to the cosine of the latitude of the place or of the inclination of the

canal to the equator, and disappearing for a sea situated at the pole: the econd part is a diurnal tide proportional to the sine of the latitude or of the inclination, being greatest when the luminary is furthest from the equinox, and vanishing when its declination vanishes."

He next proceeds "to inquire more particularly into the cause of the hitherto unintelligible fact, that the maximum of the spring tides in the most exposed situations, is at least half a day, if not a whole day, later than the maximum of the moving forces.

"Now it is easy to perceive that, since the resistance observing the lunar period is more considerable than that which affects the solar tide, the lunar tide will be more retarded or accelerated than the solar; retarded when the oscillation is direct, or when $G^2 - B$ is [negative,] and accelerated when it is inverted, or when that quantity is [positive]; and that, in order to obtain the perfect coincidence of the respective high waters, the moon must be further from the meridian of the place than the sun; so that the greatest direct tides ought to happen a little before the syzygies, and the greatest inverted tides a little after; and from this consideration, as well as from some others, it seems probable that the primitive tides, which affect most of our harbours, are rather inverted than direct."

As a convenient epoch for dating the beginning of a series of tides, it is observed that the mean conjunction, at the beginning of 1824, happens exactly at mean noon of Jan. 1, in the time of the island of Guernsey or of Dorchester, and at $18^{m}\ 49^{s}$ Parisian mean time.

It is further observed respecting the effects of resistance, that this cause "tends greatly to diminish the variation in the magnitude of the tides, dependent on their near approach to the period of spontaneous oscillation, and the more as the resistance is the more considerable: and supposing, with Laplace, that in the port of Brest, or elsewhere, the comparative magnitude of the tides is altered from the proportion of 5 to 2, which is that of the forces, to the proportion of 3 to 1; the multipliers of the solar and lunar tides being to each other as 5 to 6, . . . we find that B must be either .9380 or .6328, and the former value making the lunar tide only inverse, we must suppose the latter nearer the truth; and the magnitude of the tides will become 1.663 and 1.998, and . . A cannot be greater than .632. It seems probable, however, that the primitive tides must be in a somewhat greater ratio than this of 2 to 1, and 5 to 3, when compared with the oscillations of the spheroid of equilibrium; and if we suppose $B = .9$, and A still $= \frac{1}{10}$, we should have [6.364] and [8.78] for their magnitude;" so that the actual elevations would be about 6 and 19 feet respectively.

"Now ... the tangents of the angular measures of the displacement, $\frac{AG}{GG - B}$, give us 69° 50′ and 72° 40′ for the angles themselves, when $B = .6328$; and

if $B = .9$, these angles become 45° and 70° 24′ respectively; the difference in the latter case, 25° 24′, corresponding to a motion of more than 24 hours of the moon in her orbit.

"It appears then that, *for this simple reason only*, if the supposed data were correct, the highest spring tides ought to be A DAY LATER than the conjunction and opposition of the luminaries; so that this consideration obviously requires to be combined with that of the effect of a resistance proportional to the square of the velocity, which has already been shown to afford a more general explanation of the same phenomenon."

It may easily be admitted that this theory may require much further illustration, and perhaps discussion, before it can be rendered very popular, or intelligible, in all its bearings; but in point of mathematical evidence, it may not be superfluous to insert here the reduction of the expression of the force acting on an oblique canal into the simple form which the author has adopted, without a demonstration, at the end of his paper.

Since the force $f = \sin\cos Alt. \sin(Az. + Dev.) = \sin\cos Alt. (D' \sin Az. + D \cos Az.)$; and $\sin Alt. = L \sin Decl. + L' \cos Decl. \cos Hor. \angle$; also $\sin Az. = \dfrac{\cos Decl. \sin Hor. \angle}{\cos Alt.}$; we have $\cos Alt. \sin Az. = \cos Decl. \sin Hor. \angle$, and $\cos^2 Alt. \sin^2 Az. = \cos^2 Decl. \sin^2 Hor. \angle = \cos^2 Alt. (1 - \cos^2 Az.)$ and $\cos^2 Alt. \cos^2 Az. = \cos^2 Alt. - \cos^2 Decl. \sin^2 Hor. \angle = 1 - \sin^2 Alt. - \cos^2 Decl. \sin^2 Hor. \angle$; whence $\cos Alt. \cos Az. = 1 - \frac{1}{2} (\sin^2 Alt + \cos^2 Decl. \sin^2 Hor. \angle) + \frac{3}{8} (\sin^2 Alt. + \cos^2 Decl. \sin^2 Hor. \angle)^2 - \frac{5}{16} \ldots$; and finally,

$f = (L \sin Decl. + L' \cos Decl. \cos Hor. \angle)(D' \cos Decl. \sin Hor. \angle + D [1 - \frac{1}{2} (\sin^2 Alt. + \cos^2 Decl. \sin^2 Hor. \angle) + \frac{3}{8} \ldots])$; which may readily be more completely developed if required.

But for a lake obliquely situated at the equator, when $L = 0$, and $L' = 1$, the expression becomes $\sin Alt. = \cos Decl. \cos Hor. \angle$, and $\cos^2 Alt. \cos^2 Az. = 1 - \cos^2 Decl. \cos^2 Hor. \angle - \cos^2 Decl. \sin^2 Hor. \angle = 1 - \cos^2 Decl. = \sin^2 Decl.$, and $\cos Alt. \cos Az. = \sin Decl.$; whence

$f = \cos Decl. \cos Hor. \angle (D' \cos Decl. \sin Hor. \angle + D \sin Decl.) = D' \cos^2 Decl. \sin\cos Hor. \angle + D \sin\cos Decl. \cos Hor. \angle$, which is the equation given at the end of the Article, agreeing with the equation of the form for a canal running east and west, in having for each luminary a semidiurnal tide which is greatest when the declination vanishes, and a diurnal tide increasing, on the contrary, as the sine of twice the declination increases. The two formulæ give the same result for a canal coinciding with a part of the equator, and they appear in other cases to represent the force for every part of the same oblique great circle, the deviation at the equator being equal to the latitude when it becomes perpendicular to the meridian.

LAPLACE, assisted by the indefatigable BOUVARD, has lately published a very valuable continuation of his Researches on the Tides, as a XIIIth Book of his

Mécanique Céleste, Febr. 1824. He has computed the results of about 6000 observations made at Brest since the year 1806, and has found them confirm in general those which he had obtained from the more ancient observations. There are also some new deductions, which may be made subservient to the further illustration of the principles laid down in the Supplement of the Encyclopædia.

"I have considered," says Mr. Laplace, (P. 160,) "the tide of which the period is about a day. By comparing the differences of two high and two low waters, following each other, in a great number of solstitial syzygies, I have determined the magnitude of this tide and the time of its maximum, for the port of Brest. I have found its height very nearly one fifth of a metre, and one tenth of a day for the time that it precedes the time of the maximum of the semidiurnal tide. Though its magnitude is not one thirtieth of that of the semidiurnal tide, yet the generating forces of both these tides are nearly equal, which shows how differently their magnitude is affected by accidental or extraneous circumstances. This will appear the less surprising, when we consider that if the surface of the earth were regular and entirely covered by the sea, "*the diurnal tide would disappear, provided that the depths were uniform throughout.*" In fact, the observed heights of the diurnal and semidiurnal tides are .2134^m, and 5.6^m respectively, (P. 227); and the time that the diurnal tide precedes the maximum of the evening semidiurnal tide is .095^d, (P. 226). It is not quite clear that the words might not relate to the maximum resulting from the most perfect combination of the solar and lunar diurnal tides; but we may suppose, for the sake of the calculation, that the high water of the joint diurnal tide generally happens a little more than two hours earlier than that of the semidiurnal tide.

Now supposing, for the determination of the multiplier, $\frac{B}{\sqrt{[(GG-B)^2+AAGG]}}$ we assume the mean value of G, for the joint semidiurnal tide, about .98, and for the diurnal .49, B being about .9, and $A=.1$, the formula becomes $=7.83$, or if $A=.2$, 4.4 for the semidiurnal, and 1.327 or 1.231 respectively for the diurnal, and $\frac{\text{D}}{\text{D}'}$ or $\frac{\text{L}}{\text{L}'}$ must be such that D sin 2 *Decl.* $\times$ 1.327 may be to D$'$ $\times$ 7.83 as .2134 to 5.6, or as 1 to 26.25; but sin 2 *Decl.* = sin 46° 55$'$.5 = .73045, and we have D $\times$ .9691 : D$'$ $\times$ 7.83 = 1 : 26.25 = D : D$'$ $\times$ 8.07 and D : D$'$ $= 1 : \frac{26.25}{8.07} = 3.25 =$ cot 17° 6$'$, which must be the obliquity of the canal to the equator if $A=.1$, or if $A=.2$, 10° 30$'$: either of which may possibly be near the truth, though the obliquity of the main channel of the Atlantic to the equator is probably greater. With respect to the times of high water, the tangents $\frac{\beta}{\alpha} = \frac{AG}{GG-B}$ become, if $A=.2$, at 72° 59$'$ and at 8° 27$'$ respec-

tively; the former expressing the acceleration of the inverted semidiurnal tide, and the latter the retardation of the direct diurnal tide, by the effect of friction, the sum of the former and twice the latter is 89° 53′, or very nearly a right angle; so that the interval, thus computed, instead of one tenth of a day should be a little more than an eighth. It would, however, be necessary to compare the height of the water at different intervals before and after high water, in order to obtain the progressive magnitude of the diurnal tide with sufficient accuracy to allow us to place any reliance on the result of this computation.

With respect to the disappearance of the diurnal tide in an ocean of equable depth, no doubt the depth *must* be *equable* in order that it may disappear, but it must *also* be *evanescent*. In fact, it is not conceivable in what other manner the equability of depth can possibly produce such an effect; for there is no natural nor assignable relation between the period of revolution and that of diurnal tide; the effects are just the same as if the earth were at rest, and the attracting body moved round in a day, or in two days: and it is impossible to admit the accuracy of any refined method of investigation, from which Mr. Laplace has obtained a result so clearly contradictory to the first principles of mechanics.

ii. *An easy Method of comparing the Time indicated by any Number of* CHRONOMETERS *with the given Time at a certain Station.* By the Rev. FEARON FALLOWS, M.A., F.R.S., Astronomer at the Cape of Good Hope.

LET a transit instrument, or even a sextant with an artificial horizon, be established in a *conspicuous* situation on shore, where a clock can always be regulated to true time: then provide a powerful Argand's lamp with a shutter, so as to be able to darken the lamp instantaneously; a few minutes before a certain hour in the evening, notice being previously given to the ships, let the lamp be lighted, and at the proper instant of time let it be darkened: this may be repeated several times at short known intervals. Then the errors of every chronometer on board of all the ships, from which the lamp can be seen, are immediately found. After a certain number of days, let the same be repeated, when the daily *ship rates* will be given, since they are only the differences of these errors divided by the number of days elapsed between the two sets of observations. It is evident that for greater truth these observations may be repeated at pleasure. No objection can be made from

the chronometer being generally below deck, as one person might have his eye upon it, and another immediately above him on the upper deck might give a stamp with his foot the instant the lamp is darkened.

The longitude of Cape Castle appears from eclipses of Jupiter's satellites to be about 18° 21′ E.

The height of Table Mountain above the sea was found,

Entrance from the narrow passage on the top (5 obs.)	3430 F.
Highest western point (13 obs.)	3536
Highest eastern point (11 obs.)	3545

iii. *Easy* APPROXIMATION *to the difference of* LATITUDE *on a* SPHEROID.

SUPPOSING the excess of the equatorial semidiameter to be known and equal to e, while the semiaxis is $= 1$, and having the linear dimensions of a portion of a perpendicular to the meridian, we may compute the difference of latitude and of longitude of its two extremities by considering the case of a sphere touching the surface of the spheroid in the given parallel of latitude, and having the same curvature with the perpendicular to the meridian at its origin, which must therefore be extremely near to the points that require to be compared with each other, so that they may be supposed to be in the surface of this sphere.

Now the local semidiameter will always be $1 + e \cos^2 L$, L being the true latitude, whence, by taking the fluxion, we obtain for the tangent or the sine of the inclination of the surface, or the correction of the latitude, $2e \sin \cos L$, consequently the sine of the corrected or geocentric latitude will be $\sin L - 2e \sin \cos L \cos L = \sin L (1 - 2e \cos^2 L)$. Hence we find, by trigonometry, the normal to the equatorial plane $(1 + e \cos^2 L) \sin L (1 - 2e \cos^2 L) : \sin L = (1 + e \cos^2 L)(1 - 2e \cos^2 L) = 1 - e \cos^2 L$, e being a small fraction; and the normal to the axis, which is the radius of curvature of the perpendicular circle at its origin, $= (1 + e \cos^2 L) \cos (L - 2e \sin \cos L) : \cos L$; but $\cos (L - 2e \sin \cos L) = \cos L + 2e \sin \cos L \sin L = \cos L (1 + 2e \sin^2 L)$ and this normal

becomes $= (1 + e \cos^2 L)(1 + 2e \sin^2 L) = 1 + e + e \sin^2 L$. But the radius of curvature of the meridian is equal to the cube of the former normal divided by the square of the semiparameter, (Lyon's Fluxions, P. 111,) or to $(1 - 3e \cos^2 L)(1 + 2e)$ since the semiparameter is $\frac{1}{1+e}$, that is, to $1 + 2e - 3e \cos^2 L$, neglecting the square of e as inconsiderable.

The angle at the pole on this tangent sphere will be the true difference of the longitude on the spheroid, and the true difference of latitude may be found by reducing the angular difference of the leg and hypotenuse into linear measure, and then again into an arc of the curvature of the meridian; or more simply, by applying to the arc a correction proportional to the difference of the radii of curvature $1 + e + e \sin^2 L$, and $1 + 2e - 3e \cos^2 L$, which is $e - e \sin^2 L - 3e \cos^2 L = 2e \cos^2 L$, which is the excess of the radius of the perpendicular above that of the meridian, vanishing, as it ought to do, at the pole, and becoming $2e$ at the equator.

If it be required to compute the effect of the deviation of the perpendicular to the meridian from the plane here supposed, it may be found by making the actual verse sine of the portion of this curve in question radius, and finding the tangent of the difference of the curvature of the two circles here compared in an arc equal to the difference of latitudes found, which will be to the whole angular difference as $2e \cos^2 L$ to 1. But since the verse sine in question is only equal to the depression of the horizon at the given distance, it is obvious that the tangent of so small an angle, with this radius, may be neglected as inconsiderable.

iv. *Extract of a Memoir on the Theory of* MAGNETISM, *read at the Academy of Sciences,* 2 Feb. 1824. *By* MR. POISSON.—*Ann. de Chimie,* Feb.

IT has been customary with many natural philosophers to explain the phenomena of electric attractions and repulsions by attributing them to two distinct fluids, possessed of the property of repelling the particles of the same nature, and of attracting, with an

equal force, particles of the opposite nature: and the law of this force, as deduced from direct observation, is such, that it varies in the inverse ratio of the squares of the distances, the same law that governs the Newtonian attraction, which seems to prevail with regard to all the actions of bodies that are sensible at great distances. Setting out from this hypothesis, they have determined, by mathematical analysis, the distribution of the electricity at the surface of conducting bodies, the electrical pressure which takes place, from within to without, at every point of this surface, and the action of the electric stratum which envelopes it on any given point of space. The results of such calculations have been found in perfect conformity with the numerous experiments made by Coulomb on this subject about forty years ago, [as well as with the still earlier experiments of the late Lord Stanhope, and of Mr. Cavendish, and with the subsequent experiments of Professor Robison, and others;] and at present this part of the science of electricity, which relates to the equilibrium of the two fluids at rest, abstracted from the proper action of the substance of the electrified bodies, is theoretically complete; or at least it presents us only with such analytical difficulties, as depend on the form and the number of bodies subjected to each other's mutual influence.

From the analogy of the phenomena of magnetism with those of electricity, it was natural to attribute also the attractions and repulsions of magnetic substances to two imponderable fluids, a *boreal* and an *austral* fluid; and Coulomb inferred from his experiments the same law of inverse proportion of the square of the distances for magnetic as for electrical forces. The proofs, however, which he has adduced in support of this law, as far less conclusive for magnetism than for electricity, although there is still reason to admit its truth, so far as it may be confirmed by the agreement of calculations, rigorously derived from it, with actual experiment.

Besides the analogy of the law of the forces, there is another point of resemblance in the theories of magnetism and of electricity; that is, the distinction of bodies into two classes with regard to the greater or less degree of permanence with which they retain any

given state of magnetic or electric properties which they have been made to assume. With regard to electricity, the bodies called *conductors* are instantly electrified by the influence of neighbouring bodies already electrified, and when this influence is removed, they no longer retain any trace of electricity. Nonconducting bodies, on the contrary, are not sensibly electrified by this influence, except when it is very powerful or very long continued; but when they have once been electrified by any other means, they preserve, at each point, the electricity which has been once established in it, retaining it by a peculiar power of the matter of which they are composed. In this respect, such bodies, as are capable of magnetism, present us with a similar diversity; some of them, as soft iron, for example, which has neither been twisted nor screwed, are rendered magnetic by the influence of a neighbouring magnet; and when they are removed from it, they no longer exhibit any signs of magnetism: others, such as hard steel, are very little susceptible of this temporary influence; but if magnetism has been once excited in them by more powerful means, they preserve this state of magnetism, and, without doubt, by virtue of some particular action which their particles exert on the boreal and austral fluids.

Such are the principal analogies that are readily observed between electricity and magnetism; but on the other hand, there exist between these two affections of bodies some essential differences which must be mentioned, and which prevent the immediate application of the theory of electricity to the phenomena of magnetism.

Electricity affects all bodies, whether as passing freely through them, or as being attached to their particles: on the contrary, there are only a small number of bodies, such as iron in its different states, steel, nickel, and cobalt, in which distinct traces of magnetic action have been observed. Hence it has become a question, whether magnetism is a particular fluid, found only in bodies suceptible of its influence, or if it is merely a modification of the electric fluid, distributed in a particular manner. This question can scarcely be decided in the present state of the science; all that has hitherto been

proved is, that magnetism may be developed in different bodies by the action of electricity; but the identity of the fluids is not necessarily proved by the important facts, which have lately been discovered, relating to their connexion. Happily the decision of the question is not necessary for the purpose of this memoir, which is independent of the intimate nature of the boreal and austral fluids; its object being simply to determine the results of their attractions and repulsions, and the laws of their distribution in magnetized odies.

On this point the opinions of natural philosophers have not always been uniform. Before the time of Coulomb, the two fluids were supposed to be tranferred, by the act of magnetizing a needle, to its two ends, which thus became opposite poles; while in the opinion of this illustrious experimenter, the boreal and austral fluids are actually displaced in a very minute degree only, and do not even quit the particles of the body to which they originally belonged before it was magnetized. This opinion, however singular at first sight, has yet of late years generally prevailed: but the theory depending on it could not be correctly developed without an elaborate analysis, as will appear in the sequel of this memoir.

The general fact by which this opinion is supported, and which indeed appears to establish its truth without any reasonable doubt, is this: if we bring a magnet near to a piece of soft iron, the iron will be magnetized by induction, and the two substances, when in contact, will adhere to each other more or less strongly. The same will happen to one or more pieces of iron brought near the first; they will also be magnetized by induction, and will adhere to the first piece. If we then separate the different pieces, and remove them from the magnet, we shall find that they have all returned to their natural state, and that no portion of the magnetic fluid has passed either from the magnet into the iron, or from one piece of iron to another. Now this is a marked difference between magnetism and electricity in conducting bodies; for the electric fluid passes freely from one of these bodies to another when they are in contact, or even when they are sufficiently near for the electric

pressure to overcome the pressure of the air which confines the fluid to the surface. This fact is universally true, and is independent of the form or magnitude of the pieces of iron which are in contact, and of the degree of their magnetism, or of the force of the magnet which influences them: however intimate the contact may be, and however long it may have lasted, the fluid never passes from any one piece into another; whence it is natural to infer, that no sensible quantity of magnetism is ever transported from one part to another of the same piece of iron; and that the boreal and austral fluids, contained by the metal in its natural state, undergo insensible displacements only within it, when they are separated from one another by any exterior action. This conclusion is also equally applicable to those magnetized bodies, which retain the magnetism, that has been excited in them, either by the continued influence of a strong magnet, or by other means: the only distinction between these substances and soft iron is, that there exists in them a force peculiar to each substance, known by the name of the coercive force, of which the effect is, to arrest the particles of both fluids in the situations which they occupy, and to oppose in this manner first the separation of the two fluids, and then their return to their natural union.

A question occurs, relating to this subject, which does not appear to have hitherto excited the attention of natural philosophers, which arises however very naturally from considering the magnetic fluid as always belonging to the same constituent particles of the magnetized bodies. It is not only not demonstrated that this fluid is identical with the electric fluid, but it is not even necessary to suppose that the phenomena of magnetism are produced in all bodies by a fluid possessing every where the same intensity of attractive or repulsive action, and therefore requiring to be considered as the same fluid in different substances. The identity of the electric fluid is shown by its passing from one conducting body into another, in such a manner as to preserve all its properties, and to exercise, in the same circumstances, the same attractions or repulsions; but no such test as this can be applied in the case of magnetism, and we cannot decide from mere reasoning, whether

we ought to consider the magnetism of two different bodies, of pure nickel and of soft iron, for example, as the same imponderable substance. It is therefore from experiment alone that we can learn whether, neglecting the effect of the coercive force, which is very small in both cases, the same exterior cause will produce the same effect on the magnetic fluid contained in both these metals; or to speak more precisely, whether similar and equal needles of iron and of nickel, when submitted to the magnetic influence of the earth, or of any other magnet, would make in equal times an equal or unequal number of oscillations. Mr. Gay Lussac has been so good as to furnish me with an answer to this question, which he has obtained by substituting, for the experiment here proposed, another not less conclusive, which he considers as more capable of an accurate result.

A magnetized needle, about eight inches in length, was made to vibrate horizontally near the direction of the magnetic meridian; and by the action of the earth alone it made 10 oscillations in 131 seconds: a prismatic bar of soft iron of nearly the same length with the needle, nearly $\frac{3}{4}$ of an inch wide, and $\frac{1}{18}$ of an inch thick in a vertical direction, was fixed at the distance of two inches below the needle, and in the plane of the magnetic meridian: the oscillations of the needle were immediately accelerated, so that there were at first 10 in 65 seconds, and soon afterwards 10 in 60 seconds; but they did not become more frequent afterwards. The bar of soft iron was then exchanged for a similar and equal bar of pure nickel; and the needle made at first 10 oscillations in 78 seconds, and after some time 10 in 77 seconds; and when the bar of nickel was removed, the needle returned very nearly to its original state, making 10 oscillations only in 130 seconds, by the action of the earth alone. The bars of iron and nickel did not exhibit any perceptible magnetism after the operations, so that the coercive force, if it existed at all, must have been very weak in them: it might, however, be concluded, that it was not absolutely wanting, because the bar did not arrive immediately at the maximum of their action on the needle; but this circumstance may also have depended on the action of their magnetism on that of the needle, which required a certain

interval for producing its greatest effect, on account of the coercive force of the hard steel of which the needle was made. However this may be, we must certainly conclude from the experiment, that the mutual action of the magnetic fluids, contained in the steel and the soft iron, is decidedly greater than the mutual action of the fluids belonging to the steel and the nickel.

Perhaps it may be thought that this difference of the actions of the magnetic fluid, in the different substances containing it, may depend on the different quantity of the boreal and austral fluids contained in each of these substances when they are in the neutral state; the quantity being greater, for example, in iron than in nickel. But this view of the subject is contrary to the phenomena, the quantities of both fluids contained in each substance when neutral being without limit, as far as regards our experiments: that is to say, the forces which we command are never sufficient to exhaust or separate them by the process of magnetizing; for when a body is magnetized by the influence of a neighbouring magnet, it is admitted that the intensity of its magnetic state, as shown by the effects which it produces, increases without limit in proportion as we increase the force of the magnet employed; which implies evidently that we have not reached the limit of the decomposition or separation of the neutral fluid which it contains, in the same manner as we find it impossible to separate completely the two electric fluids contained in a conductor of electricity.

We must therefore necessarily suppose that the mutual action of two magnetic particles, belonging to different bodies, depends on the matter of each of these bodies. It is probable that this action varies also with their temperature; which seems, indeed, to follow from an old observation of Mr. Canton, and from some more extensive and more accurate experiments left unpublished by Coulomb, and since inserted in the *Traité de Physique* of Mr. Biot. These experiments show the influence of heat in the developement of magnetism; but having been made with magnetized bars, which were by no means free from coercive force, the effects observed were derived, without doubt, in part from the variation of their force, and in part from that of the magnetic action. It would therefore be

important to repeat them with soft iron and with pure nickel at different temperatures, and even with other metals, which have not hitherto been found to be subject to magnetical action: and, in fact, the experiment of Mr. Gay Lussac, which has established the difference of the action of the magnetic fluid in different substances, may afford some reason to suspect, that the intensity of this action is only very weak at ordinary temperatures, though perhaps not absolutely wanting, in other metals.

After having explained the hypothesis, or rather the physical foundations of the question which forms the subject of this memoir; we must endeavour to ascertain precisely in what manner we can represent, from these principles, the disposition of the boreal and austral fluids in magnetized bodies.

Let us first suppose that the substance is a cylindrical needle of soft iron, of a very small diameter, and of any finite length; and that, in the direction of its axis produced, there are one or more centres of magnetic action. In the natural state of the needle the two fluids contained in it are united in equal quantities throughout its substance; so that their actions being equal and opposite in all distances, they destroy each other completely, and no sign of magnetism is exhibited. The effects of the centres of magnetic action on the two fluids will separate them from each other; but each boreal or austral particle will be very little removed from its primitive situation; and in this new state the two fluids will succeed each other alternately throughout the length of the needle, and this length will consist of a series of very small parts, each of which will contain, as in the neutral state, the two fluids in equal quantities. It is unnecessary to inquire whether the length of these parts is equal to that of the constituent molecules of the iron; it is sufficient for our calculations that their length should be very minute, so that it may be neglected in comparison with the diameter of the needle, and, in general, with the smallest dimensions of the magnetized bodies which are to be considered. However small this length may be supposed, it is still conceivable that it may be different for the different substances which are capable of being magnetized, as for iron and for nickel: but it will appear in the

sequel of this memoir, that this difference would not have any sensible effect in the exterior magnetic action of the substances; so that it would not serve to explain the difference of the actions which they exert, in the same circumstances, on magnets placed in their neighbourhood.

If now we consider the case of a magnetized body of indeterminate form and dimensions, we must attend to the lines or directions in which the separation of the two fluids takes place throughout its substance, and in which they are arranged alternately, as in the needle which has been taken for an example. These lines will in general be curves depending on the form of the body, and on the external forces which act on the two fluids: they may be termed *lines of magnetization,* and we may call the minute parts, of which they are composed, magnetic elements, each containing the boreal and austral fluids in equal quantities. Thus, in each particular problem, we shall have to determine, for each point of the body to be considered, the direction of the line of magnetization, [or polarity,] and the action of the magnetic element on any other point given in position within or without the body. This action is the difference of the forces exercised by the two fluids contained in the element, arising from the slight separation of the boreal and austral molecules in the state of polarity. It is somewhat surprising to see that forces depending on distances so small, between the centres to which they belong, should be capable of producing mechanical effects so manifest, as those which result from magnetic attractions and repulsions; but in fact the result of the action of all the magnetic elements of a magnetized body is a force equivalent to the action of a very thin stratum covering the whole surface of the body, and formed of the two fluids, the boreal and the austral, occupying different parts of it. Now we are well acquainted, in the attractions and repulsions of the conductors of electricity, with mechanical effects, sometimes very powerful, which are produced by strata of fluids of a thickness so inconsiderable as to escape our senses and all our means of appreciating it. As to the ultimate magnitude of the forces which we are thus required to attribute to each of the two separate portions of fluid, whether boreal or austral, belonging to

the same magnetic element, they must be incomparably greater than the observed action of the element, and we can form no idea of their magnitude from that of the magnetic attractions or repulsions which they occasion, since these effects are only derived from their difference.

It is this distribution of the two magnetic fluids in magnetized bodies, such as it has been here described, that is to be the subject of the mathematical analysis contained in the sequel of this memoir.

The first problem that was to be resolved was, to reduce to three rectangular directions the results of all the attractions and repulsions of the magnetic elements of a magnetized body, of any imaginable form, upon a point either within or without its surface. By adding to these results, as belonging to any point within the body, those of the external magnetic forces which act on the body, we shall have the whole forces which tend to separate the two fluids that are united at the point in question. And if the matter of the body opposes no sensible resistance to the displacement of the fluids in each magnetic element, or, in other words, if there is no coercive force, it will be necessary, in order that there may be an equilibrium, that all the attractions and repulsions should destroy each other; since if any of them were uncompensated, they would produce a new decomposition of the neutral fluid, which is never exhausted, and the magnetic state of the body would be changed. The sum of the results is therefore made equal to zero with respect to each of the three directions to which they are referred. The equations of equilibrium thus formed will always be possible; they will serve to determine, for each point of a magnetized body, the three unknown quantities which they comprehend; that is, the intensity of the action of a magnetic element on a given point, and the two angles which determine the corresponding direction of the line of polarity. At the extremities of each element these joint results will not vanish, they will produce pressures, from within each element, tending outwards, and counterbalanced by the obstacle of which the nature is unknown, but which opposes the passage of the fluid from one element to another. Whatever this

obstacle may be, it is found in the superficial particles as well as in the interior, so that there is no need of any external pressure, like that of the air, to prevent the further removal of the fluid; and this circumstance constitutes a material difference between the state of induced magnetism and that of the induced electricity of a conducting substance.

If the coercive force of the magnetized body required also to be taken into consideration, it would then be sufficient for the magnetic equilibrium that the result of all the exterior and interior forces acting upon any point of the body should no where exceed the given magnitude of the coercive force, the effect of which would be similar to the friction of a machine. In this case the equilibrium might take place in an infinity of different manners; but among all these possible states, there is one which is particularly remarkable, and in which bodies are said to be *saturated with magnetism*: a case which may hereafter be made the subject of a separate memoir; the present essay being intended to comprehend only the laws of bodies magnetized by induction only, and without any coercive force.

The equations of magnetic equilibrium, formed in the way that has been described, are at first somewhat complicated; but by means of certain transformations, the triple integrals which they contain are reduced to double integrals, and the equations become much more simple. We then deduce this general consequence from them, that notwithstanding the boreal and austral fluids are distributed throughout the mass of a body magnetized by induction, the attractions and repulsions which it exercises externally are the same as if it were merely covered by a very thin stratum formed of the two fluids in equal quantities, and such that their total action upon all the points within them should be equal to nothing. If the body contains an empty space within it, and if there are centres of magnetic force within this space and without the body, it must be considered as terminated by two thin strata, corresponding to the exterior and interior surface, and the action of these two strata on any point of the substance, joined to that of all the given

centres of magnetic action, must produce a perfect equilibrium: in this case, the two fluids may be in different quantities in each of the thin strata, provided that they be always in equal quantities in the two surfaces taken together. In this manner the theory of magnetic attractions and repulsions is reduced to the same principles, and is made to depend on the same formulas with the theory of electric forces in conducting bodies; being only a particular case of these problems. But in the case of electricity the proposition here laid down is the original foundation of the theory, while in magnetism, on the contrary, it is a consequence deduced from the equations of equilibrium obtained by means of other considerations.

It may also be remarked that, according to this general proposition, if we had a collection of small masses of metal, or of any conductor of electricity, of dimensions so small that they might be neglected in comparison with those of the whole collection or aggregate, and each being surrounded by a substance capable of restraining the passage of the electricity from one to the other, but not sensibly adding to their volumes; and supposing the aggregate thus constituted to be brought near any electrified body; it would then become electrical by induction, and in this state, the attractions and repulsions, which it would exert externally, would be the same with those of a simple conducting body of the same form, subjected to the same external forces, although in the one case the two electric fluids would be transferred to opposite extremities of the body, and in the other they would be obliged to remain in the constituent masses to which they originally belonged. This supposed constitution of an electrical body is well calculated to give us a distinct idea of the disposition of the two magnetic fluids in a magnetized body.

Upon applying the general formulas of this memoir to the case of a hollow sphere, of which the solid part is of an uniform thickness, a remarkable theorem is deduced from them, which is applicable both to magnetism and to electricity. Suppose such a sphere to be formed of a conductor of electricity, and suppose electrified

bodies to be distributed in any manner whatever within or without the hollow sphere; the sphere will be electrified by induction and the effect will be such that:—

1. When all the electrified bodies are without the hollow sphere, their action, joined to that of the sphere, will give a result EQUAL TO ZERO for all the space WITHIN THE CONCAVITY, as well as for the solid part of the sphere.

2. When, on the contrary, all the electrical bodies are placed in the interior concavity, the result of their action joined to that of the sphere, on a point without, will be A CONSTANT FORCE all around the body AT EQUAL DISTANCES from its centre, and the same as if the whole of the two electric fluids were united in this point. The thickness of the electric stratum will be the same in all the extent of the exterior spherical surface, notwithstanding the different distances of its different parts from the electrified bodies within; and if the electricity passes by a spark from one of these bodies to another, or into the spherical shell, the exterior attractions and repulsions will not be changed.

WITH REGARD TO MAGNETISM, IT FOLLOWS FROM THIS THEOREM THAT A MAGNETIC NEEDLE PLACED IN THE INTERIOR OF A HOLLOW SPHERE OF SOFT IRON, AND SO SMALL AS NOT TO EXERT ANY SENSIBLE INFLUENCE ON THE SPHERE, WILL NOT BE SUBJECT TO ANY MAGNETIC ACTION, AND WILL CONSEQUENTLY NOT EXHIBIT ANY POLARITY FROM THE EFFECT OF THE EARTH'S MAGNETISM, OR FROM THAT OF ANY OTHER MAGNETS PLACED WITHOUT THE HOLLOW SPHERE. It follows also that if magnets are placed within such a sphere, their action on a small needle without it, joined to that of the shell itself as magnetized by their influence, will always produce a result equal to zero; for, from the second part of the theorem, the exterior action must be the same as if the boreal and austral fluids were both united in the centre of the sphere; which would neutralise their action at all distances, since these fluids are always necessarily present in equal quantities. And if we consider a plane as a sphere of infinite radius, we may infer that the interposition of a plate of soft iron of any given thickness, but of a great extent,

must be sufficient to prevent the transmission of the magnetic action; so that a strong magnet being placed on one side of such a plate, and at a great distance from its extremities, a small piece of iron placed on the opposite side would neither be attracted nor repelled; and on this side they would not adhere to the plate of iron, while they might adhere strongly to the ide next the magnet, although the thickness of the plate, or the distance between these surfaces, might be very inconsiderable. [It is to be presumed that this corollary is demonstrated in the original memoir; for it is by no means self evident that the remoter portion of the supposed infinite sphere would be without all effect, notwithstanding its infinite distance, especially as it has been before observed that the thickness of a stratum of electricity would be the same in every part of the spherical surface.]

The most simple case, to which the formulas of this memoir can be applied, is that of a hollow sphere, magnetized by the action of the earth, that is to say, by the action of a force of which the origin is very remote, and which may be considered, for this reason, as constant in magnitude and in direction throughout the extent of a magnetized body of ordinary dimensions. In this case, the integrations are capable of being expressed in a finite form: the equations of magnetic equilibrium are completely resolved, and we obtain from them all that is required to be known, either with regard to the direction of the lines of polarity, and to the intensity of the magnetism, in the solid part of the sphere; or with regard to the action which it exerts externally upon any point given in position. This memoir contains the expression of the three orthogonal component forces of this external action, whence it was easy to infer, by adding them to the terrestrial force, as resolved in a similar manner, the true directions of the horizontal needle, and of the dipping needle, as well as the duration of their oscillations in a given position, which will afford the simplest manner of comparing the theory with experiment. *Although the magnetism is not confined to the exterior surface of the hollow sphere, and although its intensity may be determined for any point of the solid shell, yet the magnitude of the three component forces produced by it is* WHOLLY INDEPEN-

DENT OF THE THICKNESS OF THE METAL: it is determined only by the radius of the external surface, and by the co-ordinates belonging to the position of the point on which the forces act. When the distance of this point from the centre of the sphere is very great in comparison with the radius, each of the three forces is very nearly as the cube of the radius directly, and as the cube of the distance inversely. We may reduce them to two, one directed to or from the centre of the sphere, the other coinciding with the direction of the dipping needle. The former vanishes when the point of action is situated in the plane passing through the centre of the sphere, and perpendicular to the latter: hence it follows that if a small magnetic needle be placed in this plane, the direction which it would assume, in virtue of the action of the earth, will not be altered by the attraction of the sphere. We must, however, be careful to avoid inferring from this circumstance that this attraction vanishes in the supposed plane; for the second elementary force does not vanish at the same time with the first; it will be subtracted from the action of the earth, and its effect will be to retard more and more the oscillations of the needles, in proportion as the needle is brought nearer and nearer to the sphere. At the surface itself, and in any plane intersecting it, this force is equal and contrary to the action of the earth; so that in this situation the little needle will only be urged in the direction of the radius: and in the plane perpendicular to the dipping needle, and very near the surface of the sphere, the needle would be exempt from all magnetic action, and would have no determinate direction, provided, however, that it were so small as to have its influence on the magnetism of the sphere inconsiderable.

Mr. Professor Barlow, of Woolwich, has lately made a great number of experiments on the deviations of the compass, and of the dipping needle, produced by the influence of a sphere of iron magnetized by the influence of the earth. His observations are recorded in his *Essay on Magnetic Attractions*, 2 ed. Lond. 1823. They have enabled him to conclude that the effect on the needle is the same whether the sphere that produces them is completely solid or hollow: and at the actual distances of the needle from the sphere,

he has found that the tangent of the angle of horizontal deviation is proportional to the cube of the quotient of the radius of the sphere divided by the distance of the needle from its centre: results which obviously afford a confirmation of the theory here laid down. (He has also observed that the deviations vanish when the middle of the needle is in a plane passing through the centre of the sphere, and perpendicular to the dipping needle: but *he is incorrect in calling this plane the plane of no attraction:* there is indeed no plane in which the attraction of a sphere, or in general, of any body magnetized by the earth's influence, becomes evanescent). In order, however, to compare the theory still more precisely with observation, a part of the deviations, which Mr. Barlow has determined, have been calculated from the formulœ of this memoir: and the general agreement of the results, with his observations, appears to leave no doubt either of the accuracy of this theory, or of that of the analysis which is founded on it. Without entering into the whole detail of this comparison, it will be sufficient to mention some particular cases.

The diameter of the sphere of iron magnetized by the action of the earth being thirteen English inches; and the needle, of which the deviations were observed, being six inches long, and its middle point twelve inches from the centre of the sphere; Mr. Barlow found, in a certain relative situation of the compass, a horizontal direction of 36° 15′: in the same relative position, taking into consideration the length of the needle, which is here too great to be neglected, the computation gives 35° 33′ for the same deviation: and the difference of 42′ must be partly attributed to the reaction of the needle on the sphere, which could not be comprehended in the computation, because the magnetic force of its poles is unknown.

In a continuation of the same radius, the middle point of the needle being twenty inches from the centre, the horizontal deviation was reduced to 8° 52′ by observation; the calculation would make it 8° 42′, differing only 10′ from the experiment.

At the same distance of twenty inches, and when the needle was situated near the plane in which the horizontal deviation completely

vanishes, this deviation became 1° by observation; while the calculation would make it 59′, which is a nearer agreement than could have been expected.

If we suppose two planes perpendicular to the magnetic meridian to pass through the centre of the sphere, the one horizontal, the other parallel to the dipping needle, the horizontal deviations of the compass in these two planes will have, according to the theory, a very simple relation to each other: when the right line joining the middle of the needle and the centre of the sphere makes the same angle in either plane with their common intersection, the tangent of the deviation in the horizontal plane will be to the tangent of the deviation in the other plane, as the cosine of the dip at the place of observation to unity. Mr. Barlow's observations sufficiently show the truth of this proposition: thus when the middle of the needle was eighteen inches from the centre of the sphere, the experiment gave in the second plane, at the distance of 45° from the line of east and west, a horizontal deviation of 12° 6′, and the dip being 70° 30′, it would be inferred that the corresponding deviation in the horizontal plane should be 4° 6′, while the observation made it only 4°: the difference of 6′ being probably owing to the errors of observation.

The same mode of computation has been applied to the dip, as observed by Mr. Barlow under the influence of the sphere of iron; and the differences are not greater than are usually found in all such comparisons. Thus when the dipping needle was placed in the plane of the magnetic meridian, passing through the centre of the sphere, and at the distance of twenty inches, making an angle of 45° with the direction of the earth's magnetism, the dip was reduced from 70° 40′ to 67° 40′; the calculation gives 67° 46′ which differs only 6′ from the result of the experiment.

In these numerical computations it has been supposed; first, that the action of the earth is the same on the magnetic fluid of the sphere magnetized by its influence, and on the fluid belonging to the needle employed; secondly, that the action of the fluid of the sphere on itself is also equal to the action that it exerts on that of the needle. It was natural to make these suppositions in the first

instance, and the differences between the calculation and the observations are not sufficiently great to induce us to abandon them. Besides, if there were any slight difference of intensity among these actions, depending on the difference of the materials of which the sphere and the needle are formed, the observations in question could scarcely have been sufficiently accurate to determine so delicate a point.

The author terminates the abstract of this admirable memoir with a remark which he thinks may be of some advantage in practice.

The horizontal deviation of the needle, produced by the influence of the magnetized sphere, and the relation of the number of oscillations which it makes when so influenced to the number of its spontaneous oscillations, comprehend, in their analytical expressions, that of the dip at the place and time of observation: so that by making the deviation and the ratio of the variations equal to the values obtained by observation for a known situation of the needle, we may obtain two equations, either of which might serve to compute the dip. If we employ the ratio of the oscillations, we have the advantage of being able to obtain it by observation, with sufficient accuracy, from a needle so small as to be incapable of sensibly affecting the magnetism of the sphere. The equation to be resolved, in order to obtain the dip, will contain the diameter of the sphere, and the distance from its centre; both which may be measured with great precision: it will also comprehend the two angles which determine the line of direction of the needle from the sphere: but when the needle is near the point which affords the maximum of action, a small error in the direction of this line will have little influence on the magnitude of the dip, which may be determined by the method here described with more accuracy and facility than by direct observation.

A second memoir is intended to contain a determination of the mode of distribution of magnetism in needles of steel magnetized to saturation, and in needles of iron magnetized by induction, by means of the same general theorems that have been demonstrated in the present essay; and from these distributions the phenomena of their mutual attractions and repulsions will be deduced, upon similar principles.

Art. XIII. ANALYSIS OF SCIENTIFIC BOOKS.

I. *Meteorological Essays and Observations*, by J. Frederick Daniell, F.R.S. London: Underwoods. 1823. 8vo. Pp. 479. Three Plates.

"Man," as Mr. Daniell has correctly observed at the commencement of the work before us, "may almost with propriety be said to be a meteorologist by nature; he is actually placed in such a state of dependance upon the elements, that to watch their vicissitudes and anticipate their disturbances, becomes a necessary portion of the labour to which he is born. The daily tasks of the mariner, the shepherd, and the husbandman, are regulated by meteorological observations; and, the obligation of constant attention to the changes of the weather, has endued the most illiterate of the species with a certain degree of prescience of some of its most capricious alterations. Nor, in the more artificial forms of society, does the subject lose any of its universality or interest: much of the tact of experience, indeed, is blunted and lost; but artificial means, derived from science, supply, perhaps inadequately, the deficiency; and the general influence is still felt and acknowledged, though not accurately appreciated. The generality of this interest is indeed so absolute, that the common form of salutation amongst many nations is a meteorological wish, and the first introduction between strangers a meteorological observation."

The important modifying influence exerted over the human frame by different conditions of the atmosphere; the comparative hilarity and corporeal energy communicated by one variety of weather, and the languor and oppression experienced in another, have long attracted the regards of philosophers to the investigation of the origin of many of those diseases, especially of an epidemic nature, which affect mankind. Hitherto, however, but little positive information has been derived from the inquiry; the precise physical condition—the exact *constitutio aëris*, exerting a baneful influence over health, being still enveloped in uncertainty.

The various eudiometrical experiments which have been instituted in sickly climates and seasons having failed to elucidate the subject, the same constituent gaseous principles, and the same proportion of those constituents having been found as in a healthy atmosphere, it has been supposed by the author before us, that an accurate method of estimating the varying quantity of aqueous vapour in the elastic medium which surrounds us—the only fluctuating ingredient of its composition, might lead to some useful hints on this interesting subject, and suggest in some important diseases, in those of the lungs

more especially, the construction of an artificial atmosphere, of greater efficacy than any that has hitherto been recommended.

It is to be feared, however, that such knowledge might not lead to results as valuable as might at first be imagined. It would not seem that air highly charged with aqueous vapour, if unaccompanied with excessive heat or cold, or noxious exhalations, is remarkably injurious to human health. In the marshy districts not only in this country, but universally, consumptions are comparatively rare, and considerable benefit has been derived from sending phthisical individuals from dry and lofty districts, into others where the atmosphere has been more charged with humidity. Pisa is chiefly on this account one of the most genial climes in the south of Italy for consumptive subjects, although in other respects extremely unhealthy, owing to the malaria exhaled by the surrounding marshy districts. Were accurate registers, however, taken of the comparative barometric, thermometric, and hydrometric variations, and the corresponding states of public health or disease correctly registered, as might be readily done in some of the large valetudinarian establishments of this country; considerable advantage would inevitably follow, if not in a therapeutical at all events in an hygienic point of view. Unfortunately, meteorology has heretofore been but little studied as a science, and although many of its parts have been ably elucidated by several existing philosophers, amongst whom the author of the volume before us stands especially conspicuous, yet it must be considered to be still in its infancy.

The three first sections of this scientific production are occupied by an elaborate disquisition on the constitution of the atmosphere, of which it is impossible for us to give more than a recapitulation of the principal conclusions to which the author has arrived, after a patient investigation of the researches of the most eminent philosophers, conjoined with the results of his own observations.

The grand conclusions are as follows:—There are two distinct atmospheres, mechanically mixed, surrounding the earth, whose relations to heat are different, and whose states of equilibrium, considering them as enveloping a sphere of unequal temperature, are incompatible with each other. The first is a permanently elastic fluid, expansible in an arithmetical progression by equal increments of heat, decreasing in density and temperature according to fixed ratios, as it recedes from the surface, and the equipoise of which under such circumstances, would be maintained by a regular system of antagonist currents. The second is an elastic fluid, condensable by cold with an evolution of caloric, increasing in force in geometrical progression with equal augmentations of temperature: permeating the former and moving in its interstices, as a spring of water flows through a sand-rock. When in a state of motion, this intestine filtration is retarded by the *inertia* of the gaseous medium, but in a state of rest the particles press only upon those of their own kind.

The density and temperature of this fluid have also a tendency to decrease, as its distance from the surface augments by a rate less rapid than that of the former. Its equipoise would be maintained by the adaptation of the upper parts of the medium, in which it moves, to the progression of its temperature, and by a current flowing from the hotter parts of the globe to the colder. Constant evaporation on the line of greatest heat, and unceasing precipitation at every other situation, would be the necessary accompaniments of this balance. The conditions of these two states of equilibrium, to which, by the laws of hydrostatics each fluid must be perpetually pressing, are essentially opposed to each other. The vapour or condensible elastic fluid is forced to ascend in a medium, whose heat decreases much more rapidly than its own natural rate: and, it is consequently condensed and precipitated in the upper regions. Its latent caloric is evolved by the condensation, and communicated to the air; and it thus tends to equalize the temperature of the medium in which it moves, and to constrain it to its own law. This process, the author considers, must evidently disturb the equilibrium of the permanently elastic fluid, by interfering with that definite state of temperature and density which is essential to its maintenance. The system of currents is unequally affected by the unequal expansion, and the irregularity extended, by their influence, much beyond the sphere of the primary disturbance. The decrease of this elasticity above, is accompanied by an extremely important re-action upon the body of vapour itself, being compelled to accommodate itself to the circumstances of the medium in which it moves, its own law of density can only be maintained by a corresponding decrease of force below the point of condensation; so that the temperature of the air, at the surface of the globe is far from the term of saturation; and the current of vapour which moves from the hottest to the coldest points, penetrates from the equator to the poles, without producing that condensation in mass, which would otherwise cloud the whole depth of the atmosphere with precipitating moisture. The clouds are thereby confined to parallel horizontal planes, with intermediate, clear spaces, and thus arranged are presented to the influence of the sun, which dissipates their accumulation, and greatly extends the expansive power of the elastic vapour. The power of each fluid being in proportion to its elasticity, Mr. Daniell considers that of the vapour compared with the air can never exceed at most 1.30; so that the general character of the mixed atmosphere is derived from the latter, which in its irresistible motions must hurry the former along with it. The influence, however, of the vapour upon the air, though slower in its action, is sure in its effects, and the gradual and silent processes of evaporation and precipitation govern the boisterous power of the winds. By the irresistible force of expansion unequally applied, they give rise to undulations in the elastic fluid; the return-

ing waves dissipate the local influence, and the accumulated effect is annihilated, again to be produced.

"In tracing the harmonious results of such discordant operations," eloquently observes our author, "it is impossible not to pause, to offer up a humble tribute of admiration of the designs of a beneficent Providence, thus imperfectly developed in a department of creation where they have been supposed to be the most obscure. By an invisible, but ever active agency, the waters of the deep are raised into the air, whence their distribution follows, as it were by measure and weight, in proportion to the beneficial effects which they are calculated to produce. By gradual, but almost insensible, expansions, the equipoised currents of the atmosphere are disturbed, the stormy winds arise, and the waves of the sea are lifted up; and that stagnation of air and water is prevented which would be fatal to animal existence. But the force which operates is calculated and proportioned: the very agent which causes the disturbance bears with it its own check; and the storm, as it vents its force, is itself setting the bounds of its own fury. The complicated and beautiful contrivances by which the waters are collected "above the firmament," and are at the same time "divided from the waters which are below the firmament," are inferior to none of those adaptations of INFINITE WISDOM, which are perpetually striking the inquiring mind, in the animal and vegetable kingdoms. Had it not been for this nice adjustment of conflicting elements, the clouds and concrete vapours of the sky would have reached from the surface of the earth to the remotest heavens; and the vivifying rays of the sun would never have been able to penetrate through the dense mists of perpetual precipitation."—P. 132.

The reference to this admirable and complicated agency by which the different constituents of the atmosphere are so beautifully and regularly balanced, leads the author to the consideration of a subject which has always been a favourite with the sceptic, and on which we must necessarily continue to remain in considerable doubt and conjecture: still, the philosophical explanation which Mr. Daniell adopts, along with Mr. Granville Penn, to whom he expresses himself indebted for the first idea of it, appears to us the most probable of any that has been propounded, and the most consistent with those principles which are known to regulate the aërial fluid.

"The question has been asked," says the author, "How is it that light is said to have been created on the first day, and day and night to have succeeded each other, when the sun has been described as not having been produced till the fourth day? The sceptic presumptuously replies, this is a palpable contradiction, and the history which propounds it must be false. But Moses records that God created on the first day the earth covered with water, and did

not till its second revolution upon its axis, call the firmament into existence. Now, one result of the previous inquiry has been, that a sphere unequally heated and covered with water, must be enveloped in an atmosphere of steam, which would necessarily be turbid in its whole depth with precipitating moisture. The exposure of such a sphere to the orb of day would produce illumination upon it, that dispersed and equal light, which now penetrates in a cloudy day, and which indeed is "good:" but the glorious source of light could not have been visible from its surface. On the second day the permanently-elastic firmament was produced, and we have seen that the natural consequences of this mixture of gaseous matter, with vapour, must have been, that the waters would begin to collect above the firmament, and divide themselves from the waters which were below the firmament. The clouds would thus be confined to definite plains of precipitation, and exposed to the influence of the winds, and still invisible sun. The gathering together of the waters on the third day, and the appearance of dry land, would present a greater heating surface, and a less surface of evaporation, and the atmosphere during this revolution would let fall its excess of condensed moisture: and, upon the fourth day it would appear probable, even to our short-sighted philosophy, that the sun would be enabled to dissipate the still remaining mists, and burst forth with splendour upon the vegetating surface. So far, therefore, is it from being impossible that light should have appeared upon the earth before the appearance of the sun, that the present imperfect state of our knowledge will enable us to affirm, that, if the recorded order of creation be correct, the events must have exhibited themselves in the succession which is described. The argument, therefore, recoils with double force in favour of the inspiration of an account of natural phenomena, which in all probability, no human mind, in the state of knowledge at the time it was delivered, could have suggested; but which is found to be consistent with facts that a more advanced state of science and experience have brought to light." P. 134.

The important modifying influence exerted over atmospheric phenomena by the electric fluid and the moon, are not entirely passed over by our author in his interesting inquiry, although he has not been able to add any thing to our existing stock of knowledge on the subject: from those experiments, however, instituted by him, he is inclined to believe that the elasticity of vapour is increased when electrically charged, but on this point he has nothing decisive to offer. The popular and general opinion of the different phases of the moon possessing an influence over atmospherical vicissitudes which has been denied by some philosophers, and considered as the offspring of superstition and ignorance, is attentively considered by the author, and accorded with. Innumerable observations have shewn that such a relationship does actually exist, and it is not at all more extraordinary than the influence exerted over the tides by that satellite.

Of the next essay, "*On the Construction and Uses of a New Hygrometer,*" we shall say but little; the ingenious ideas which led to its adoption: its mechanism and uses have been already detailed in this Journal, by the author, and the practical observations made with it in different portions of the globe, communicated by different scientific individuals through the same channel. To those, however, who have not perused Mr. Daniell's description of the instrument, the essay before us will afford every necessary information regarding its construction, and mode of employment.

The author complains, and not without justice, of the difficulties experienced in "approaching the shrines whence the oracles of science are issued," and relates the following anecdote which we wish were unique: unfortunately, it is not the first instance by many where obstructions have been experienced in the fair investigation of philosophical discoveries by the academy in question, and frequently, we fear, from an overweening desire to promulgate the discoveries of their countrymen, and a corresponding apathy towards those of other nations.

"Being actuated," says Mr. Daniell, "by the wish to obtain contemporaneous observations, and to do all in my power to facilitate so desirable an object, and my own opinion being confirmed by those whose judgment I could not doubt, I took an opportunity of sending by a private hand, two of the hygrometers, in their most perfect state, to one of the philosophers of the French Royal Academy of Sciences, the most distinguished for chemical knowledge and discoveries. I requested his opinion of the merits of the instrument, and authorized him to present one of them, in the most respectful way to the Academy. My presumption has, I suppose, been p operly checked, by no notice whatever having been taken of what was certainly meant as a mark of humble respect, either by the individual, or the learned body: to the former of whom, having had the advantage of a personal introduction, I cannot feel that I have been to blame in addressing myself, however small may have been my pretensions for obtruding myself upon the latter." P. 185.

The next essay to which we shall advert, comprises a dissertation on the climate of London; a subject not less interesting to the lover of meteorology, as a science, than to the physician; there are, however, so many circumstances independently of the exact condition of the atmosphere as regards temperature and dryness, which exert a baneful influence over human health, that these phenomena are not so decisive in the study of causation as might *à priori* be imagined: it has indeed been asserted by some philosophers that the greater salubrity of one country over another is principally owing to the lesser degree of noxious emanations from its soil; and Heberden, Blane, and others, have affirmed that on the whole, except in the case of extraordinarily cold winters, the fluctuations of the weather in this climate do not much affect health. Still epidemics do occur,

which there is every reason to suppose have been occasioned by atmospherical changes, and consequently, as we have observed in a former part of this article, if these variations, barometrical, thermometrical, and hygrometrical, were carefully and regularly arranged at corresponding periods, and the changes in the condition of public health accurately marked, some very interesting and valuable information might in all probability be obtained, not less important to the physician than to mankind in general.

We can only enumerate here these general characters of the climate as adduced by Mr. Daniell, on an average of three successive years. The monthly phenomena are given, accompanied with popular observations on the corresponding conditions of the weather, the state of vegetation, health, *&c.*: but for those we must refer the reader who may be anxious to peruse them, to the work itself.

The observations were made three times a day, *viz.*, from eight to ten o'clock A. M., from half past three to half past five P. M., and from ten to half past eleven P. M.

The mean pressure of the total atmosphere as denoted by the barometer was found to be 29.881 inches: the mean of twenty years, deduced by Mr. Howard from the observations of the Royal Society being 29.8655 inches. The mean temperature derived from the daily *maxima* and *minima* of the thermometer was 49°.5, corresponding even to the decimal place with Mr. Howard's estimate. The mean dew-point was 44°.5, as also calculated from the daily *maxima* and *minima*. The elastic force of the vapour was consequently 0.334 inch, and a cubic foot of the air contained 3.789 grains of moisture. The degree of moisture was represented by 5° upon the thermometric scale, and the degree of moisture by 850 upon the hygrometric. The average quantity of rain was 22.199 inches, and the amount of evaporation calculated from the hygrometer, 23.974 inches; and the weight of water, raised from a circular surface of six inches diameter, 0.31 grains per minute.

The Barometric range was from 30.82 inches to 28.12 inches: the range of the dew-point from 70° to 11°. The pressure of the vapour varying with it from 0.770 inch, to 0.103 inch. The *maximum* temperature of the air was 90°, the *minimum* 11°. The force of radiation from the sun averaged 23°.3 in the day, and that from the earth at night 4°.6: the highest temperature of the sun's rays was 154°, and the lowest temperature on the surface of the earth 5°. The greatest degree of dryness was 29°, or the least degree of moisture upon the hygrometric scale 389. The time of the day was found, in some degree, to influence the near results; and one of the most constant effects was that produced upon the barometer. The mercurial column reached its greatest height in the morning, declined to its lowest in the afternoon, and again rose at night. The average difference of these periods, as exhibited by the journal, was as follows:—Morning above night +.005 inch; afternoon below morning —.015 inch;

night above the afternoon +.010 inch. The means of the monthly observations presented but one or two exceptions to the fall in the middle of the day, or to the rise from afternoon to night; but the rise from night to morning was not quite so constant.

With regard to the dew-point, four observations were made daily, including the observation of the *minimum* temperature, which constantly falls a few degrees below the term of precipitation taken in the day. From morning to afternoon it was found to rise but 0.3 of a degree; from afternoon to night it fell 0.9 of a degree; and below this again, the *minimum* temperature was 2.7. The mean was calculated from this and the afternoon observation.

The temperature of the air was found to vary in the twenty-four hours from 56°.1, its mean *maximum* to 42°.5 its mean *minimum*.

"The mean temperature of a climate," says our author, "is generally regarded as made up of the average impression of the sun due to its latitude upon the surface of the globe. The mean quantity of aqueous vapour must also be referable, finally, to the same principle. But there is another way of considering the subject more accurate in detail, though upon an average of years ending in the same conclusion: that is, to regard the mean temperature as made up of the temperature of different currents flowing from different points of the compass; and it will be necessary to my purpose to contemplate the atmosphere of vapour particularly, in this point of view. The medium dew-point 44°.5 is therefore made up of the following proportions of the means from eight points of the wind:—

87	North	40°.1	—	133	North-east	40°.7
80	East	42.3	—	111	South-east	45.6
70	South	48.7	—	225	South-west	48.6
215	West	44.8	—	174	North-west	41.3

"Before I enter upon the consideration of the effect of the sun's progress in declination, and the succession of the seasons, I shall endeavour to point out the influence of the geographical situation of the island of Great Britain upon its aqueous atmosphere. The mean quantity of the vapour follows exactly the changes of the mean monthly temperature, that is to say, the dew-point rises and falls with the increase and the decrease of the heat. But the winds which transport the vapour, may be divided into two classes; namely, the land-winds which blow from off the great continent of Europe, and which comprise the north-east, the east, and south-east; and the sea-winds which blow from the great oceans which surround it on every other side, *viz.*, the north, north-west, west, south-west, and south. In the former we may expect to find that the course of the mean temperature is exactly followed; for the sources of the vapour must be comparatively shallow streams, and reservoirs of water, whose temperature must soon adapt itself to that of the surrounding air.

But, in the unfathomable depths which supply the latter, the law by which the density of water is regulated, must, at particular seasons, maintain a temperature above the mean of the declining season; whilst at others, the increasing heat of the latter must outstrip the progress of the former. The following Table contains the dew-point of the several winds, divided into the two classes for every month in the year, beginning with the autumnal quarter.

TABLE I. *Shewing the Difference of the Dew-point in the Land and Sea Winds.*

	Land Winds N.E. E. S.E.	Sea Winds. N. N.W. W. S.W. S.
	°	°
September	53	53
October	45	46
November	41	42
December	31	37
January	29	35
February	31	35
March	34	38
April	45	42
May	47	44
June	54	54
July	52	55
August	56	57

"And here the effect anticipated is clearly perceptible. The vapour of the land winds, it will be seen, declines in force from September to January, in which month it reaches its minimum, and from that point gradually rises till it reaches its maximum in August; and this, it will be afterwards seen, is the exact progress of the mean temperature of the air. In the sea-winds the vapour follows the same course from September to November, and the balance is such, that the elastic force of both divisions is nearly the same. The north and south winds neutralize each other; and the north-west, west, and south-west, are equivalent to the north-east, east, and south-east. Having descended to about 40°, which is somewhere about the point of greatest density in water, in November, the accordance proceeds no further. In December, the vapour from the land has descended six degrees below that from the sea, and the difference continues in January. In February the former rises two degrees, and the latter remains stationary. The difference of four degrees continues through March, and is diminished to three degrees in April and May. In June they again attain their former equality. The reason of this is obvious; the temperature of 40° being that of the greatest density, cannot be lowered till the whole mass of the waters has passed this term; and in the

deep seas, this must necessarily be a process of some duration. The shallow waters, on the contrary, soon assume the temperature of the ambient air, and continue to decline with it in heat. Upon the return of spring the contrary effect is produced. The great deeps must again repass the fortieth degree before the superficial waters can take the higher temperature of the incumbent atmosphere. The consequences we should expect from this progression, would be an increase of humidity in December and January, and a rapid decrease in the four following months; an expectation which we shall find correct in our further investigation.

"There is another law of the aqueous fluid, which we might also expect to have an influence upon the emission of its steam—the evolution, namely, of heat in the process of congelation, and its absorption during the liquefaction of ice. The British Isles are placed in such a position as would induce us to suppose that, at particular seasons of the year, this influence might be perceptible in one direction more than in any other. We may bring this idea to the test, by comparing together the northerly and southerly winds, as is done in the following table:—

TABLE II. *Shewing the Effect of the Ice in the North Seas upon the Dew Point.*

	Southerly S.W. S. S.E.	Northerly N.E. N. N.W.
	°	°
September	58	48
October	51	41
November	47	37
December	42	32
January	38	31
February	36	31
March	42	32
April	47	40
May	51	41
June	58	50
July	58	50
August	60	54

"Here we may observe, that the decline of the vapour from September to December is exactly equal in both classes, but from that time it ceases about the temperature of 32° in the northerly winds, and continues in the southerly to the month of February. In March, again, the temperature of the latter has increased from the minimum 6°, but in the former it still remains at 32°. In April, on the contrary, the increase in the northerly winds exceeds that of the southerly; and in May, they have again attained their original relative

distances and resume their parallel progression. It would be difficult, I think, to assign any other cause for this modification of the phenomena than the one which has just been suggested. The evolution of heat, in the process of freezing, stops the decline of the temperature in the regions exposed to its influence, while it proceeds in those which are not exposed to the change; and the absorption of heat in the operation of thawing, prevents the accession of temperature which is due to the returning influence of the sun. When this operation has ceased, the vapour quickly attains its former relative degree of force. Wonderful adjustments these, to mitigate the rigours of a northern climate! They both operate from November to February, by the evolution of heat in the coldest season of the year; and at the same time, by an extra supply of vapour, decrease the degree of dryness, and prevent the consumption of heat which always attends the process of evaporation." P. 273.

The next essay to which we shall draw attention relates to a subject of a more practical nature, and comprises some information of considerable utility to the meteorologist; it is entitled, "*Remarks upon the Barometer and Thermometer, and the Mode of using Meteorological Instruments in general.*" Than Mr. Daniell no one is more competent to furnish valuable hints on this matter, from a considerable portion of his attention having been given to the manufacture of barometers. The Committee of the Royal Society, appointed to take into consideration the state of the meteorological instruments, did the author the honour to request him to attend to the construction of a new barometer for their apartments, and in the course of this inquiry he had, of course, an opportunity of making many extremely valuable practical observations.

In the course of the experiments, he was led to a new method of filling the tube, of greater facility and correctness; for the particulars of which we must refer to the book itself. It consists in conducting the process *in vacuo,* and the author has but little hesitation in considering it as accurate as the method of boiling, if performed with proper care, whilst it is infinitely less troublesome and hazardous. The electric light is as strong in the tube, and its appearance, in every respect, as perfect.

The following remarks on the faulty construction of meteorological instruments in general, are extremely just and important.

"The generality of observers are but little aware of the serious inaccuracies to which those instruments are liable. In the shops of the best manufacturers and opticians I have observed that no two barometers agree; and the difference between the extremes will often amount to a quarter of an inch; and this with all the deceptive appearance of accuracy, which a nonius, to read off to the five hundredth part of an inch can give. The common instruments are mere playthings, and are, by no means, applicable to observations in the present state of natural philosophy. The height of the mercury is never

actually measured in them, but they are graduated one from another, and their errors are thus unavoidably perpetuated. Few of them have any adjustment for the change of level in the mercury of the cistern, and in still fewer is the adjustment perfect: no neutral point is marked upon them, nor is the diameter of the bore of the tube ascertained; and in some the capacity of the cisterns is perpetually changing from the stretching of a leathern bag, or from its hygrometric properties. Nor would I quarrel with the manufacture of such play things; they are calculated to afford much amusement and instruction; but all I contend for is, that a person, who is disposed to devote his time, his fortune, and oftentimes his health, to the enlargement of the bounds of science, should not be liable to the disappointment of finding that he has wasted all, from the imperfection of those instruments, upon the goodness of which he conceived that he had good grounds to rely. The questions now of interest to the science of meteorology require the measurement of the five hundredth part of an inch in the mercurial column; and, notwithstanding the number of meteorological journals, which monthly and weekly contribute their expletive powers to the numerous magazines, journals, and gazettes, there are few places, indeed, of which it can be said that the mean height of the barometer for the year has been ascertained to the tenth part of an inch. The answer of the manufacturer to these observations is, that he cannot afford the time to perfect such instruments. Nor can he, at the price which is commonly given; for few people are aware of the requisite labour and anxiety. But who would grudge the extra remuneration for such pains? Not the man who is competent to avail himself of its application. Let the manufacture of playthings continue, but let there be also another class of instruments which may rival in accuracy those of the astronomer. It will, no doubt, be a part of the plan of the Committee of the Royal Society to establish a standard barometer, and to afford every facility of comparison with it: so that any person, for scientific purposes, may have an opportunity of verifying an instrument; and it is to be hoped that they may proceed one step further, and take measures for ascertaining the agreement of the instruments at all the principal observatories, not only in this country, but in other parts of the world.

"Nor is it in the construction of barometers only that the meteorologist has to complain of that want of accuracy which is so essential to the progress of his science; the same carelessness attends the manufacture of the thermometer. Few people are aware that they are all, even those which bear the first makers' names, made by the Italian artists, who graduate them one from another, and never think of verifying the freezing and boiling points. The bulbs are all blown with the mouth, and very little attention is paid to the regularity of the tube. The register thermometers are particularly shamefully deficient. Those of Six's construction are often filled with some saline solution instead of alcohol; and in the best, the spirit is not exposed

long enough *in vacuo*, to disengage the air with which it is mixed. The consequence is, that it is liable to become liberated, and, of course, interferes with the results. The original directions of the inventor have also been departed from, as to the proportions of the different parts, and as to the construction of the *indices*. Those upon Rutherford's plan are universally sealed with air in their upper parts, which acts as a spring against the expansion of the column: the iron index of one is liable thereby to become oxidated, and adheres to the glass when the mercury passes it, and it becomes entangled; while the spirit of the other being unavoidably mixed with air, when the pressure is decreased by cold it is disengaged. The air may be again dissolved by increasing the pressure before a fire, and passing the bubble backwards and forwards, and, in a state of solution it does not appear to interfere with the equability of the expansion. This, however, is not certain; and, at all events, it is liable to re-appear, and is very troublesome. These imperfections are by no means necessary consequences of the construction of the instruments, although the makers are very willing that they should be so considered; but it requires great care and attention to guard against them. The general mounting of the meteorological thermometers is exceptionable in every way; buried as they are in a thick mass of wood, and covered with a clumsy guard of brass, they can but very slowly follow the impression of atmospheric temperature. The establishment of a perfect standard thermometer, which shall be accessible to all who may wish to consult it, will also, doubtless, be another object of the Committee of the Royal Society." P. 368.

Attention to the perfection of instruments, however, as the author has very correctly observed, will be all in vain, without a proper degree of care and system in making and recording the observations. The proper hours of the day for observation are indicated by the barometer: the maximum height of the mercurial column is at about nine A. M., the mean at twelve, and the minimum at three P. M. Where an individual has time to make three observations in the day, these hours should be preferred; if he can only observe twice, the first and last hours should be the periods; and if only once, noon should be the time. Even those who merely consult the barometer as a weather-glass, would, Mr. Daniell asserts, find it an advantage to attend to those hours; for he has remarked that much the safest prognostications from this instrument may be derived from observing when the mercury is inclined to move contrary to its periodical course. If the column rise between nine A. M. and three P. M., it indicates fine weather; if it fall from three to nine, rain may be expected.

The thermometer should be inspected at the same periods, in addition to which the author recommends that the *maximum* and *minimum*, by register thermometers, should be carefully noted; the instruments should, of course, be sheltered from every kind of radiation.

The periods of the barometric observation are recommended also for those of the hygrometer; the mean pressure of the aqueous atmosphere, however, being calculated from the dew-point at three P. M., and the lowest temperature at night of the sheltered thermometer.

This Essay comprises also some interesting information on the change in the freezing point which occurs in time in the best thermometers, and has been imagined to be owing to the alteration of form and capacity which the glass undergoes from the pressure of the atmosphere upon the *vacuum* of the tube; as well as some remarks upon the correction to be applied to barometers for the expansion of mercury and mean dilatation of glass. For information on these points the reader is referred to the Essay itself.

Independently of the Essays to which we have already adverted, there are several others of very considerable interest to the philosopher contained in the volume before us; of these our limits will only admit of an enumeration of the titles; they will be found, however, not less scientific and important than those on which we have dwelt at some length. They are,—1. An Essay upon the radiation of heat in the atmosphere. 2. An Essay upon the horary oscillations of the barometer. 3. Meteorological observations at Madeira, Sierra Leone, Jamaica, and other stations between the Tropics, by Captain E. Sabine, R.A. F.R.S. 4. Meteorological observations in Brazil, and in the Equator, by Alexander Caldcleugh, Esq. And 5. Meteorological observations upon heights. The work is also concluded by an excellent meteorological journal for three years, commencing on the first of September, 1819.

After the analysis and extracts which we have given in the preceding pages, it is almost unnecessary for us to remark on the mode in which the work is executed. The various subjects, it will have been observed, are treated of in a manner highly creditable to the talents and scientific acquirements of the author; whilst the language is in general elegant and perspicuous; the reasoning forcible; and the propositions, drawn from principles premised, are logical. To the lover of meteorological science in particular, as well as of natural philosophy in general, these Essays will be found to form a rich mine of new and important information.

II. *A Translation of the Pharmacopœia of the Royal College of Physicians of London, 1824. With Notes and Illustrations. By* Richard Phillips, F.R.S. L. and E. *&c., &c.*

Considering the materials he has had to work upon, Mr. Phillips has really given us a very useful book, in his translation, as he calls it, of the *Pharmacopœia;* and has shown something of alchemical power, in respect to the contents of the meagre original. We are well aware of the talents that exist in the College of Physicians, and are therefore utterly at a loss to account for the careless imbecility of the productions which are, from time to time, sent forth under its auspices. Where is Dr. Wollaston? where Dr. Young? What has become of Dr. Maton and Dr. Paris? have they no interest in the public character of the body which they adorn; or are they merely careless of its reputation; or do they leave so weighty a concern as the publication of the *Pharmacopœia* to the beadle and the bookseller? These are questions asked every day, and every where, and we profess our entire inability to offer to them any plausible reply. That they are not unjustly asked, we are sorry to say is but too manifest, from the present edition, which we understand to be the production of a Committee of the College; and although some tendency towards improvement is manifest in several of the processes, the general execution of the work is very unworthy of its source.

The old preface of the edition of 1809 is unaccountably reprinted, and attached to the present work; had this preface contained a history of pharmacy, or a review of former pharmacopœias, its retention might have been excusable; but it is, in fact, a poor and empty production, and particularly inappropriate to the present state of pharmaceutical science, which has lately made such rapid and important progress. To illustrate and expound this progress should have been the business of the preface, if any were thought necessary. The researches which have led us to a tolerably accurate knowledge of the substance upon which the activity of opium depends, and those which have taught us the existence of distinct salifiable bases in the greater number of narcotic vegetables; the inquiries instituted with so much success respecting the principles upon which the active powers of the varieties of Cinchona depend; and those which have taught us the importance of iodine, and some of its combinations, in the treatment of glandular diseases; all these subjects should have been touched upon in the preface, if preface there needs must be; we ought also to have been informed why the college have not introduced any of these new and active substances; whether they consider them ineffectual, or dangerously active; why they have altogether passed them by; why they have retained in the list of their *Materia Medica,* sorrel and wood-sorrel, marsh-mallow and coltsfoot, bistort and cuckoo-flowers, centaury, contrayerva and cow-

hage, carrots, raisins and figs, bay-berries and mulberries, opoponax and sagapenum, storax, oyster-shells and toxicodendron; why, in short, so much of the old lumber is suffered to encumber this new work, while so many useful novelties, which have a place in foreign pharmacopœias, are omitted. We are fully aware of the mischief and absurdity of stuffing every new crudity into a pharmacopœia; the Parisian codex amply proves that; but when we know that all apothecaries are obliged to keep sulphate of quinina and hydriodate of potash, and acetate of morphia, and that several Fellows of the College, justly eminent for their skill and extensive practice, prescribe and have faith in these compounds, there are, we think, grounds for the questions we have humbly submitted. Our experience, however, obliges us to admit that there must be some hidden obstacles and unseen difficulties in the way of compiling a good and rational pharmacopœia; for, taking it all in all, that of the London college is perhaps the best extant. Whether to the prevalence of a pugnacious diathesis, and the impossibility of deciding, when doctors disagree; or to the want of co-operation among scientific and practical men, or to what other cause we are to attribute this fatality, we shall not now stop to inquire; perhaps those who have access to the minute-book of the Committee of the College, are the only persons who can solve the problem.

Like ancient Gaul, the *Pharmacopœia* is divided into three parts: one assigned to some preliminary matters respecting weights and measures; the second to the *Materia Medica;* and the last to the preparations and compounds. We shall follow Mr. Phillips' example in passing over the two former divisions without remark. The third is subdivided into sections, of which the first treats of "Acids," alphabetically arranged.

The term "diluted acetic acid" is properly enough applied to distilled vinegar, but the process of distillation might well have been rejected; for all medical purposes a dilute acid, composed of 1 part of the concentrated acetic acid, contained in the *Materia Medica*, and four parts of water, is preferable. Of this mixture, or of distilled vinegar, the sp. gr. should be about 1009, and 1000 parts should saturate 145 of crystallized carbonate of soda: 50 grains of *real* acetic acid saturate, according to our translator, 153 grains of this salt, and upon this datum the following is the composition of the dilute acid of different specific gravities:

Sp. Grav.	Real Acid.	Water.
1007	3.42	96.58
1009	4.73	95.27
1043	23.67	76.33
1046	28.43	71.57

Of these acids, the two first are the average strength of distilled vinegar, and the two last that of the concentrated acetic acid, as now generally prepared by the vinegar-makers from pyroligneous acid.

Benzoic Acid is an article which might very well be struck out of the *Pharmacopœia*; the process, however, now directed is preferable to that of the last edition.

A process for obtaining *Citric Acid* is given in this division, but it also has a place among the articles of the *Materia Medica*, and is so rarely prepared except by the manufacturer upon an extended scale, that the directions here given might well have been dispensed with. Mr. Phillips tells us that an ounce of water at 60° dissolves 10 drachms of crystallized citric acid; and such solution saturates about 20 drachms of crystallized carbonate of soda. Nine drachms and a half of citric acid dissolved in a pint of distilled water, give, he says, a solution equal in strength to lemon juice.

We shall quote the article "Muriatic acid" entire, that our readers may judge of the method which the translator pursues in his remarks and of their general usefulness to students and practitioners.

Muriatic Acid.

"Take of dried muriate of soda, two pounds,
Sulphuric acid *by weight*, twenty ounces,
Distilled water, a pint and a half;

"First mix the acid with half a pint of the water in a glass retort, and to these, when cold, add the muriate of soda; pour the remainder of the water into a receiver; then, adapting the retort to it, let the muriatic acid distil into the water from a sand-bath, the heat being gradually raised until the retort becomes red hot.

"The specific gravity of muriatic acid is to that of distilled water as 1·160 to 1·000.

"One hundred and twenty-four grains of crystallized subcarbonate of soda, are saturated by 100 grains of this acid.

"*Process.*—The nature of common salt, and the production of muriatic acid, are explained by two theories, both of which I shall state, because, from the name of muriate of soda which the college retain for common salt, it would appear that, as a body, they have not adopted the generally-received doctrines of Sir H. Davy on these subjects.

"On the supposition that muriatic acid is an undecomposed body, the explanation of its production is the following: Common salt, or muriate of soda, is a compound of muriatic acid and soda, and when it is mixed with the sulphuric acid, this, owing to its greater affinity for soda, expels the muriatic acid from it, which, being gaseous, and having considerable affinity for water, rises in the state of vapour with it, and is condensed in the receiver into liquid muriatic acid. The sulphuric acid and soda remain in the retort in the state of sulphate of soda.

"This process will be explained by the annexed diagram:

Liquid Muriatic Acid.

Muriate of Soda	Muriatic Acid.	Water.	Diluted Sulphuric Acid
	Soda.	Sulphuric Acid.	

Dry Sulphate of Soda.

"According to the opinion of Sir H. Davy, now generally adopted, common salt, or chloride of sodium, is a compound of 36 chlorine and 24 of the metallic body sodium; liquid sulphuric acid consists of 40 parts of dry acid and 9 of water, the water being composed of 1 of hydrogen and 8 of oxygen; when these quantities of common salt and liquid sulphuric acid act upon each other, the water and chloride of sodium are both decomposed; the 1 of hydrogen uniting with 36 of chlorine, constitute 37 of muriatic acid gas, and the 8 of oxygen with the 24 of sodium form 32 of oxide of sodium, or soda. The 37 of muriatic acid gas combining with the water used in diluting the acid, rise with it in the state of vapour, and by condensation in the receiver, liquid muriatic acid is produced; the 40 parts of dry sulphuric acid uniting with the 32 of soda, form 72 of dry sulphate of soda, which remain in the retort.

37 *Muriatic Acid Gas.*

60 Chloride of Sodium	36 Chlorine	1 Hydrogen 8 Oxygen — 9 Water	49 Liquid Sulphuric Acid.
	24 Sodium 8 *Oxygen* — 32 Soda	40 Dry Sulphuric Acid.	

72 *Dry Sulphate of Soda.*

"In preparing this acid it is, I think, more convenient to mix the sulphuric acid and water in a separate vessel than in the retort; to introduce the salt first into the retort and to pour the acid upon it; and to put less water into the receiver, and more into the retort.

"*Qualities.*—Muriatic acid, when perfectly pure, is colourless; it emits white suffocating fumes, which turn vegetable blues red; its taste is strongly sour and acrid; when its sp. gr. is 1·160 as directed by the college, a fluid ounce weighs about 527 grains; it is stated

that 100 grains saturate 124 of crystallized subcarbonate of soda, which, from some indirect experiments, I believe to be not quite correct. By the French chemists it is termed hydrochloric acid, to express its nature. It acts upon and dissolves several metals with the evolution of hydrogen gas arising from the decomposition of water. Thus iron, zinc, and tin are readily dissolved by it; it acts but slowly upon copper, but dissolves its oxides with facility. Its saline compounds are termed muriates, and most of them suffer decomposition when heated, as I shall explain when describing the properties of muriate of lime.

" *Composition.*—Muriatic acid gas is composed of equal volumes of hydrogen gas and chlorine gas; and the combination takes place without alteration of volume. By weight it consists nearly of

Hydrogen	2·7	or 1 atom of hydrogen	.	= 1
Chlorine	97·3	1 do. of chlorine	.	= 36
	100·0	Number representing its atom		= 37

" Liquid muriatic acid of sp. gr. 1·160 is composed of nearly 32·4 of muriatic acid gas, and 67·6 water.

" *Adulteration.*—This acid, as usually met with, has a yellow tinge, which is owing either to the presence of chlorine or of peroxide of iron; if the former be present, it may sometimes be determined by the smell, or by its power of dissolving gold leaf; the latter is detected by the addition of solution of ammonia, which, when added slightly in excess, throws down the peroxide of iron of a reddish yellow colour. It sometimes also contains sulphuric acid; this is discoverable by adding a solution of muriate of barytes to a portion of the acid diluted with 4 or 5 parts of distilled water. This dilution is requisite, because the acid, when concentrated, attracts the water from the solution of muriate of barytes, and causing it to crystallize, gives a fallacious appearance of the presence of sulphuric acid.

" *Incompatibles.*—This acid is incompatible with alkalies, most earths, oxides and their carbonates, sulphuret of potash, tartrate of potash, tartarized antimony, tartarized iron, nitrate of silver, and solution of subacetate of lead.

" *Officinal Preparations.*—Ferrum Ammoniatum.—Tinctura Ferri muriatis.

" *Medicinal uses.*—According to Dr. Paris, it may be advantageously employed in malignant cases of scarlatina and typhus, and, mixed with a strong infusion of quassia, he considers it to be the most efficacious remedy for preventing the generation of worms. Dose ♏ v.—xx. frequently repeated."

When Mr. Phillips says, " the nature of common salt and the production of muriatic acid are explained by two theories," *&c.*, and when he speaks of the " supposition that muriatic acid is an undecomposed body," and afterwards, without expressing any doubts

upon the subject, states, that it is composed of equal volumes of hydrogen and chlorine, we think that he exceedingly perplexes his subject, as far as medical readers are concerned; it is just as if he were to say that the calcination of a metal may be explained upon two theories, and then cite the phlogistic and antiphlogistic hypothesis. Sir H. Davy's chloridic theory alone furnishes a consistent explanation of the phenomena above alluded to, and we are sorry to see the blunders of the oxymuriatic school perpetuated by such a writer as Mr. Phillips, when even Berzelius has ceded. In other respects the chemical remarks of the translator are very pertinent and useful, but we could have wished for information somewhat more extended in respect to the *medicinal uses* of the different articles, and think that the list of "incompatibles" had better, in most cases, have been omitted.

Our author's remarks upon the other acids are very much to the purpose, and are studded with several originalities useful to the pharmaceutical chemist. He has made much use of diagrams, and has given wood-cuts of the usual crystalline forms; we, however, rather doubt *their* use, and are certain that neither the apprentice nor his master will ever refer to the relations of the several plane surfaces to each other, which are given with an elaborate minuteness incompatible with the general tenor of the work.

The officinal acids of the present *Pharmacopœia* are seven, *viz.*, the acetic, benzoic, citric, muriatic, nitric, sulphuric, and tartaric. Might not the hydro-cyanic have been properly added? Alkalies and their salts are treated of in the second division of this part of the *Pharmacopœia.* Mr. Phillips has unnecessarily embarrassed his observations on the subcarbonate of ammonia, by giving the wrong as well as the right theory of its formation, but in other respects his remarks upon the carbonates of ammonia are original and important. Under its medicinal uses he says that thirty grains of carbonate of ammonia are emetic, which is far from being always the case.

The College continue to apply the erroneous terms *Subcarbonate* and *Carbonate* of potass to the carbonate and bi-carbonate, but the process for obtaining the latter is materially improved by deriving the carbonic acid from carbonate of lime, instead of (as formerly) carbonate of ammonia. But, as if some fatality attended the introduction of an innovation, they direct the gas to be passed into their own *liquor potassæ*, instead of a much more dilute solution, which ought to have been employed. Mr. P. objects, we think without reason, to the use of dilute sulphuric acid for the decomposition of the powdered marble, and recommends muriatic acid as a substitute, suggesting that, for sake of economy, the muriate of lime may be decomposed by sulphuric acid, and thus dilute muriatic acid regained; but we have not found his objections to sulphuric acid hold good in practice.

The remaining salts of potash require little notice; the super-

sulphate, perfectly useless, is still retained; and of the hydriodate, of which many practitioners think very highly, not a word is said.

In respect to the salts of soda we observe the same impropriety of nomenclature in distinguishing the carbonates which has been noticed of the carbonates of potash. The *sodæ carbonas* is however, as it commonly occurs, a compound of an atom of carbonate and one of bi-carbonate, with four of water, and therefore may be called a *Sesqui-carbonate of Soda.* Mr. Phillips found the native carbonate of soda from Africa to be an analogous compound. The formula for sulphate of soda is quite unnecessary, as it is always prepared by the wholesale manufacturer.

Among the earths we observe that lime is directed to be obtained by the calcination of marble, and of shells, the use of the latter being by no means obvious; and although marble duly heated furnishes very good lime, that which may be had wholesale is as fit for the preparation of lime-water.

Proceeding to the metals and their salts, we have to congratulate the College upon the improvement in their formula for that most important compound *tartarized antimony*, which is now prepared by boiling finely-levigated glass of antimony with tartar in a due proportion of water. The exact composition of emetic tartar is not very easily determined, nor has our author given us any thing original respecting it. The hydro-sulphuretted oxide of antimony is still retained under the improper title of *Precipitated Sulphuret of Antimony;* and the very uncertain formula for the preparation of antimonial powder remains nearly as it was.

The more we consider the antimonial remedies of the *Pharmacopœia*, the more we are convinced that emetic tartar is the only certain and definite remedy of that class; that it may be used in various mixtures as a substitute for the other preparations, and that it is the only compound of the metal which *ought* to be retained in a pharmacopœia compiled upon sound principles.

The exceeding absurdity of calling certain solutions *wines* which contain *no* wine, occurs first under this head, where 20 grains of tartarized antimony dissolved in 8 ounces of water, and 2 ounces of rectified spirit, is foolishly termed *Vinum Antimonii tartarzati.* We are not generally inclined to be very sceptical upon the subject of pharmaceutical nomenclature, but this capricious innovation we cannot leave unnoticed. In the last *Pharmacopœia* the term *liquor* was learnedly applied to a real vinous solution; and now, the term *vinum* is applied to that which contains no wine. But the alteration is otherwise mischievous. Antimonial wine and steel wine are domestic remedies, with which every body is acquainted, and no vender of medicines who wished to retain his customer would think of sending out the *wines* of the present *Pharmacopœia* under that name. The apothecary therefore is obliged to hamper his shelves with both solutions, and this merely to gratify a whimsical propen-

sity for something new, which exists somewhere in the College; for after all, the present *wines*, which contain no wine, are as objectionable as the former wines which do contain it. But the authors of the *Pharmacopœia* will probably tell us that it is compiled exclusively for their own use and convenience; that they have nothing to do with the vulgar public; and that if other people are unlearned enough to call things by their proper names, they regret their want of taste. "We have thought it better," it is said in the Preface to the *Pharmacopœia*, "to risk the accusation of barbarism than to admit terms of doubtful or uncertain signification," but in the cases before us certainty might have been attained without barbarism.

It is with unfeigned regret, that we find ARSENIC is still retained in the *Pharmacopœia*. We do not mean to say that it is useless as a medicine, but we do mean explicitly to assert that the mischief of retaining it is many thousand times greater than any benefit that in any possible case can be derived from its curative powers. The only plausible excuse for the sale of arsenic is its supposed use in medicine; and as long as the College think it right to sanction its employment, so long may any person obtain it of any chemist, druggist, or apothecary—let it be struck out of the *Pharmacopœia*, and its sale prohibited, and the numerous cases of accidental and intentional poisonings with it would, at all events, be thrown off the shoulders that now alone must bear the responsibility. As to the cow-doctors and horse-leeches, (who by the way kill more cattle than they cure with their arsenical lotions,) we put them out of the question—and why then is arsenic, in every way the most dangerous, pernicious, portable, and certain of the poisons, the most easy of administration, and the most difficult of detection, suffered to be sold at every chandler's shop in the kingdom?

The College have given sub-nitrate of bismuth a place in their new *Pharmacopœia*—to this we have no objection, though we find upon inquiry that the physicians of most practice never prescribe it: we must repeat that sulphate of quinine and hydriodate of potassa should not have been neglected, for they, and especially the former, are in daily use.

Among the preparations of iron we, in the first place, observe that *Ferrum Ammoniatum* and *Liquor ferri Alcalini*, useless, uncertain, and unchemical as they are, are retained; this is a pity, for all these pharmaceutical incumbrances are, in more ways than one, prejudicial; the advantages of tartarized iron are frustrated by the directions for drying it; and our old acquaintance *steel wine*, the *vinum ferri*, has a most clumsy and inefficient substitute in a solution of tartarized iron in proof spirit. Mr. Phillips is more temperate in his remarks upon this preposterous innovation (which has already excited infinite dismay and perplexity in many nurseries) than we feel inclined to be, and we shall therefore quote his

most merciful criticism, observing by the way, that in compounding their wines the College seem to have had an inverse eye upon Mrs. Glass's *water* pudding, so called, as she facetiously tells us, because made with *wine only*.

"This preparation," says Mr. Phillips, "is tartrate of potash and iron, with excess of supertartrate of potash, which is probably intended to supply the place of the acid contained in the wine formerly employed, and to effect the perfect solution of tartarized iron in the weak spirit.

"The quantity of iron directed to be used is very nearly such, that if it were all acted upon by the supertartrate of potash, and dissolved by the spirit, the strength of the present preparation would almost exactly equal that which I found the former to possess. But three causes prevent this: first, the whole of the iron is not acted upon by the tartar; secondly, a part of that which is converted into tartarized iron, is rendered insoluble by drying; and thirdly a portion which is dissolved by the water is immediately precipitated by the spirit. I find that owing to these circumstances, a pint of the present *vinum ferri* contains only sixteen grains of peroxide, instead of twenty-two grains, which an equal quantity of the former preparation held in solution."

Among the preparations of mercury we think that the red oxide, the grey oxide, and the sulphurets, might without much inconvenience to any one have been omitted. The formula for calomel is most unequivocally improved; it is, indeed, the best extant; that for corrosive sublimate would be the better for a little alteration in the proportion of the materials. The solution of corrosive sublimate is here called *liquor* and not *vinum*, as is the case with that of emetic tartar; but it should not have been among the formulæ, for it is liable to decompose, and in remedies of such activity every thing depends upon the accuracy of the proportion held in solution. We wish the College had been prevailed upon to reject their present names for calomel and corrosive sublimate; and that Mr. Phillips had not added to the proper chloridic explanation of their composition and formation, the incorrect and exterminated muriatic hypothesis; he seems to have done it out of compliment to the College, "who," he says, "do not appear to have adopted the modern views of the nature of muriatic acid;" but no authority can justify the perpetuation of error.

The preparations of lead remain much as in the former *Pharmacopœia*, excepting that the term *sub*-carbonate is now improperly used for what before was properly called *carbonate*. The formula for acetate of lead is now no longer necessary; it is prepared of great purity, and at a low price by the wholesale manufacturer, and might therefore have been transferred to the *Materia Medica*.

The formula for oxide zinc is much ameliorated by substituting precipitation of the sulphate by ammonia, for the old process of com-

bustion; in this way it is obtained free from metallic particles. By some oversight the quantity of water directed for the solution of the sulphate is however too small.

Of the preparations of sulphur, the solution of that substance in oil, and the "precipitated sulphur," might be dispensed with.

The general directions given in the *Pharmacopœia* for the collection, preservation, and preparation of vegetables, are meagre and unsatisfactory; Mr. Phillips has merely transcribed them without any remarks. Among the distilled waters, we observe that cinnamon-water, peppermint-water, mint-water, and penny-royal-water, are directed to be distilled either from the herbs, or from their essential oils. The same rule should have been extended to rose-water, which is more fragrant and less apt to acidify when so prepared.

With very few exceptions, we think that the infusions and decoctions should have been left to extemporaneous prescriptions, especially the former; there are also many among them which certainly might have been altogether expunged. The extracts are also much too numerous; they are generally apt to spoil by keeping, and such only, therefore, should have been retained as are really useful. Under the term *extractum stramonii* we have an useless extract of the *seeds* of the thorn-apple.

Among the mixtures and spirits we also have several useless, or, at least, unnecessary formulæ; among the latter, especially, *spiritus ammoniæ fœtidus*, *spiritus ammoniæ succinatus*, *spiritus armoraciæ compositus*, *spiritus colchici ammoniatus*, *spiritus menthæ viridis*, *spiritus pimentæ*, *&c. &c.*, are mere incumbrances; if they are medically wanted, extemporaneous prescriptions with the essential oils are preferable. The "tinctures" offer a sufficiently judicious selection, but many of them might be improved by digestion for a shorter time, in a moderate heat; nothing is said of the temperature at which they should be prepared.

Under the "preparations of æther" we may remark, that the formulæ for sulphuric æther and rectified æther should have been given under one head; for what is rectified æther but sulphuric æther? or, what medical use can be made of the impure æther which the College call *æther sulphuricus?* They certainly direct it, but probably by mistake, in their spirit and compound spirit of sulphuric æther. Æthereal oil, aromatic spirit of æther, and compound spirit of sulphuric æther, as now directed by the College, are very useless supernumeraries upon this list.

The Section on Æthereal Preparations is followed by one including the wines containing no wine; and then follow the medicated vinegars, honeys, syrups, and confections.

Among the compound powders we observe many, and among the pills more formulæ, which might be dispensed with; the latter are liable to harden, and with few exceptions should never be kept *ready made*.

Under the remaining heads of the *Pharmacopœia*, including plasters, ointments, &c., we observe nothing worthy of particular remark. Mr. Phillips has added to his translation a series of woodcuts, exhibiting the most commonly occurring crystalline forms of the principal salts, &c., which, as far as they go, are useful, as being more explanatory than mere descriptions, and the pupil ought, for several reasons, to be acquainted with the ordinary figures which these bodies exhibit; but, as before observed, the measurements of angles and inclinations of surfaces which accompany the description of the salts are not, we conceive, very important to the utility of a work like this. We are also somewhat disappointed at the brevity of the original remarks and scantiness of the criticisms, where there is so much room and opportunity for both, and more especially when we advert to the diligence and acumen, sometimes perhaps a little too highly seasoned, with which our author animadverted upon the glaring errors and abundant inconsistencies of the former *Pharmacopœia.* Something more also might have been said of the medical uses and forms of prescribing the leading articles; indeed we doubt whether the present extremely concise notices culled chiefly from Dr. Paris's *Pharmacologia*, had not better have been omitted. But we must not complain: these things are not in Mr. Phillips's way, and upon the whole we are indebted to him for many useful hints and pertinent remarks.

We wish, in conclusion, to disclaim the remotest intention of disrespect towards the College in any of the remarks which we have found it necessary to offer upon their *Pharmacopœia*, and which, with all its imperfections, we have already acknowledged among the best extant. There seems, therefore, to be some hidden impediment to the compilation of a rational Pharmacopœia, and at all events it must not be assumed as a standard of the talents of its nominal editors; there must be something radically wrong in the mode of managing the matter, and before the College give us another edition, we trust they will seriously consider the subject, and adopt some less exceptionable mode of proceeding. We apprehend that the whole business should be unconditionally delegated to three or five individuals, who should alone have power, and be solely responsible: they should moreover be well paid for their trouble, and no expense should be spared in furnishing them means of information and research. The Committee which determines by vote what formulæ are to exist and what to be expunged, should certainly be broken up: the men of practical eminence in the College have no time to attend to it; and the men of science are, if we mistake not, wearied out by the persevering prosers and obstinate ancients with which all such Committees are pestered.

Art. XIV. MISCELLANEOUS INTELLIGENCE.

I. Mechanical and General Science.

1. *Adhesion of Nails in Wood.*—Mr. Bevan has published in the *Philosophical Magazine* a series of very interesting experiments on the adhesion of nails when driven into different kinds of wood, the results of which we have abstracted and condensed as below. The following table exhibits the relative adhesion of nails of various kinds, when forced into dry Christiana deal at right angles to the grain of the wood:

	Number to the lb. avoirdnpois.	inches long.	inches forced into the wood.	lbs. required to extract.
Fine sprigs	4,560	0.44	0.40	22
Ditto	3,200	0.53	0.44	37
Threepenny brads	618	1.25	0.50	58
Cast-iron nails	380	1.00	0.50	72
Sixpenny nails	73	2.50	1.00	187
Ditto			1.50	327
Ditto			2.00	530
Fivepenny nails	139	2.00	1.50	320

The percussive force required to drive the common sixpenny nail to the depth of $1\frac{1}{2}$ inch into dry Christiana deal with an iron weight of 6,275 lbs. was four blows falling freely the space of 12 inches, and the steady pressure required to produce the same effect was 400 lbs.

A sixpenny nail driven one inch across the grain into dry elm required 327 lbs. to extract it; driven end-ways, or longitudinally, it required 257 lbs. for its extraction: driven end-ways two inches into Christiana deal it was drawn by a force of 257 lbs., but driven in one inch only in the same direction, it was extracted by 87 lbs. The relative adhesion therefore, when driven transversely or longitudinally, is as 100 to 78, or about 4 to 3, in dry elm; and as 100 to 46, or as 2 to 1, in deal.

To extract a common sixpenny nail from a depth of one inch out of dry oak required . . . . . . 507 lbs.
dry beech 667
green sycamore 312

a common screw of $\frac{1}{5}$ of an inch diameter was found to have an adhesion about three times that of a sixpenny nail.—The resistance to entrance of a nail was found to be to that of extraction, in some experiments, as 6 to 5.—*Phil. Mag.* lxiii. 168.

2. *Levels in London above the highest Water-mark.*

	feet.	inches.
North-end of Northumberland-street, Strand	19	7½
North of Wellington-street, Strand	35	6
North of Essex-street, Strand	27	0
West of Coventry-street	52	0
South of St. James's-street	13	3
South of Air-street, Piccadilly	49	8
North of St. James's-street	46	7
West of Gerrard-street	61	4
North of Drury-lane	65	0
South of Berner's-street	74	3
South of Stratford-place	59	4
North of Regent-street	76	0
South of Orchard-street	70	4
North of Cleveland-street	80	10
Centre of Regent's Circus	77	2
North of Gloucester-place	72	3
North-side of Aqueduct crossing Regent's Canal	102	6
Opposite south-end of King-street, Great George-st.	5	6

The whole of Westminster, except the Abbey and part of Horse-ferry-road, is below the level of the highest tide.

N. M. Mag. xii. 206.

3. *On the comparative Advantage of Coke and Wood as Fuel.*—Some trials have been made by M. Debret on the heating power of coke and wood, when consumed in stoves, at the Royal Academy of Music. Two similar stoves were heated, one by wood and the other by coke, and the temperature of the exterior, taken at some distance from the fire. The temperature of the flues was at first 9° c., and the mean temperature, at the end of six hours, was, by the wood, 13° c., by the coke, 16° c.; so that the increase by the wood was 4°, by the coke 7°. These effects were produced by seventy-three kilogrammes, (163 pounds) of wood, worth three and a half francs, and twenty-four kilogrammes, (53 pounds) of coke, worth one franc eighty cent.

During the progress of this experiment another stove had been heated for several hours with wood, and the temperature had not risen above 13°. The use of coke very quickly raised it to 15° or 16°. Hence it is concluded, and with reason, that coke is much preferable for these purposes to wood; but where the stove is small the mixture of a little wood with the coke is recommended to facilitate the combustion.—*Bib. Univ.* xxv. 237.

4. *Vicat on burning of Limestone or Chalk.*—From some experiments formerly made by M. Vicat, that philosopher was induced to conceive, that probably an imperfect calcination of limestone would

make it yield a better hydraulic lime than a more complete burning; but having, by the lapse of time, had occasion to make further observations on the specimens of chalk mortar, which formed the subject of the experiments on which that opinion was founded, he has taken the opportunity of guarding against any such conclusion being drawn from his previous statement.—(See vol. xvi. p. 386.)

On examination of the specimens of chalk cement, four months after they were immersed in the water, they were found just in the state they were in on the twelfth day; they resisted the trial needle to a certain extent only, and not at all like a specimen of good hydraulic lime, which was put into water at the same time.

M. Vicat had occasion to make further remarks on the imperfect burning of lime, in consequence of the opportunity afforded by a large block of limestone which had been used in the construction of a kiln, and which furnished from different parts various specimens burnt in different degrees. Five varieties were selected, No. 5, and also No. 4, slacked in water, and were therefore set aside as considerably burnt. Nos. 3, 2, and 1 were not attacked by water, they were almost as hard as before burning, and being pulverized, sifted, and made into a paste, they were immediately immersed in water and left. After a month they were scarcely hardened, and were far worse than the specimens of chalk before referred to. The same stone pulverized and calcined for twenty minutes on a red hot iron, gave a cement not so good as the chalk, but better than the specimen from the furnace.

" These experiments," says M. Vicat, " are far from confirming the general results announced by M. Minard, (vol. xvi. p. 387); I can scarcely believe that we shall ever obtain, I will not say good, but even passable, cement, by the calcination, more or less complete, of pure calcareous stones. We must probably always have recourse to the argillaceous limestones, and when these are well studied and classed in proportion to the quantity of clay and lime which they contain, and that accounts are preserved in all cases of the results of the experiments, we shall perhaps be forced to acknowledge, that nothing is more advantageous than a good hydraulic lime, which yielding from 1.1 to 1.3 parts for 1, can for 100 measured parts receive 160 or 180 of sand, and thus furnish at a very moderate price a mortar equally capable of resisting the vicissitudes of the atmosphere, and the destructive effects of running water.—*Ann. de Chim.* xxv. 60.

5. *On the Application of Muriate of Lime as a Manure.*—M. Dubuc, a druggist, and member of the Academy of Sciences at Rouen, has, during the years 1820, 21, 22, and 23, made use of chloride of calcium as a manure, or according to his own expression, as a vegetable stimulant. His experiments have been numerous, and the following short notice is given of them by M. Lemaire Lisancourt.

A kilogramme (2.2 lb.) of chloride of calcium is dissolved in sixty litres (126.8 pints,) of water. The ground intended for experiments is watered with the solution; the seeds are then sown, or the plants set in the ground, and ultimately the watering is repeated a third or fourth time with the solution.

M. Dubuc sowed some Indian corn in a light soil, watered six or eight days before with the solution. At a distance of six feet, but in the same soil, and with the same aspect other maize was sown and watered with common water. The first, which was watered from time to time with the solution of the chloride, attained to double the size of the second. Specimens of both were presented to the academy at Rouen. He has also hastened and favoured the developement of the great pyramidal campanula, of the lilac, and other trees, and also of fruit-trees, *&c.* He has also made experiments on market vegetables; onions, and poppies, which grow to a large size in the soil of Rouen, have doubled in volume by the action of the chloride. He has observed the great annual sunflower rise as in Spain to a height of twelve or fifteen feet, whilst in ordinary circumstances this large herb did not rise more than six or eight feet. He has seen the stems of these vegetables three or four inches in diameter above the earth, the leaves from eighteen to twenty inches long, the discs of the flowers twelve or fourteen inches in diameter, producing seeds from which half their weight of good oil has been extracted, and ultimately exuding from their centres a transparent secretion analogous to turpentine, very odorous, and easily drying in the air.

Finally, M. Dubuc made his experiments on potatoes, taking such as in size and weight were nearly alike. These were planted May 1, 1822, in the same soil, and with the same aspect but in two beds, separated from each other by a path six feet wide. One of these beds was watered with the vegetative liquor, the other with water from a cistern. They were all gathered the 10th Nov. 1822. The first gave tubercles six inches long, twelve inches in circumference, and weighing nearly 2lbs. each; the others were generally about half that size. These large potatoes were equally nourishing with the ordinary potatoes, and were equally well preserved until the following April. They were watered only three times with the solution during the time they were in the earth, and their leaves were developed in an equal proportion.

It appears that in general it is sufficient to water the vegetables submitted to the action of chloride of calcium three or four times with the solution at long intervals. The electro-organic power of this substance seems very singular, for, as M. Labarraque, of Paris, has observed, when applied to the animal organization, it in a short time arrests the progress of gangrene, chancres, or ulcers, and powerfully favours the production of fleshy pimples, which cicatrize the sore.—*Ann. de Chim.* xxv. 214.

6. *Preparation of Caoutchouc.*—Mr. T. Hancock, has succeeded, by some process, the results of long investigation, but which he has not published, in working caoutchouc with great facility and readiness. It is cast, as we understand, into large ingots, or cakes, and being cut with a wet knife into leaves or sheets about $\frac{1}{8}$ or $\frac{1}{10}$ of an inch in thickness can then be applied to almost any purpose for which the properties of the material render it fit. The caoutchouc thus prepared is more flexible and adhesive than that which is generally found in the shops, and is worked with singular facility. Recent sections made with a sharp knife or scissors, when brought together and pressed, adhere so firmly as to resist rupture as strongly as any other part, so that if two sheets be laid together and cut round, the mere act of cutting joins the edges, and a little pressure on them makes a perfect bag of one piece of substance. The adhesion of the substance in those parts where it is not required is entirely prevented by rubbing them with a little flour or other substance in fine powder. In this way flexible tube catheters, *&c.*, are prepared; the tubes being intended for experiments on gases, and where occasion might require they should sustain considerable internal pressure, are made double, and have a piece of twine twisted spirally round between the two. This therefore is imbedded in the caoutchouc, and at the same time that it allows of any extension in length of the tube, prevents its expanding laterally.

The caoutchouc, is in this state, exceedingly elastic. Bags made of it as before described, have been expanded by having air forced into them until the caoutchouc was quite transparent, and when expanded by hydrogen they were so light as to form balloons with considerable ascending power, but the hydrogen gradually escaped, perhaps through the pores of this thin film of caoutchouc. On expanding the bags in this way the junctions yielded like the other parts, and ultimately almost disappeared.

When cut thin, or when extended, this substance forms excellent washers, or collars for stop-cocks, very little pressure being sufficient to render them perfectly tight. Leather has also been coated on one surface with the caoutchouc, and without being at all adhesive, or having any particular odour, is perfectly water tight.

Before caoutchouc was thus worked it was often observed how many uses it might in such a case be applied to; now that it is so worked it is surprising how few the cases are in which persons are induced to use it. Even for bougies and catheters it does not come into use, although one would suppose that the material was eminently fitted for the construction of these instruments.

7. *Magnetic Intensity of a Chronometer.*—A remarkable example of the magnetic intensity of a chronometer has just appeared in Vol. X., Part I., of the *Transactions of the Royal Society of Edinburgh.* Mr. Harvey, the author of the investigation, by employing

a very delicate apparatus, constructed on the principle of *Coulomb*, and capable of detecting the minutest traces of attraction, discovered very remarkable varieties of magnetic power in a time-keeper. By denoting the intensity of the terrestrial magnestism by 100, he found the intensity of the chronometer one inch above the centre of its crystal, to be respectively 90.79, 102.29, 90.69, and 78.89, according as XII was directed north, east, south, and west. By determining also the tntensity one inch below the bottom of the timekeeper, the intensities in the same directions were 77.17, 91.34, 101.26, and 94.94. In like manner Mr. Harvey found, by determining the intensities of the sides, that they were severally 105.61, 89.61, 91.78, and 84.05. The intensity also one inch above the extremity of the steel arbour of the fusee was 109.09; and in the line of a common tangent, proceeding from between the barrel and fusee, XII being uppermost, 107.82. When, however, the chronometer was turned a quadrant, so as to bring the middle of the side of the spring-box an inch below the centre of the oscillating bar, IX being uppermost, the intensity amounted only to 92.22; and over the small interval between the balance and the fusee, it fell to 79.51.

On examining the balance Mr. Harvey found the inner rims of the arcs of compensation to be of steel, and which, together with the time-screws, were in a state of active magnetism, particularly the latter, one having strong northern polarity, and the other southern. The small wormed cylinders also, on which the thermometer pieces moved, presented equal proofs of polarity, one being a north pole, and the other a south. When the north pole of a small bar magnet was placed near the extremity of the wormed cylinder which possessed northern polarity, the balance immediately receded a small quantity; but when the south pole was applied, the power was sufficient to cause it to advance through a minute but sensible arc; and similar effects were produced when the proper poles of the magnet were presented to the extremity of the wormed cylinder having southern polarity. On presenting a more powerful magnet, the balance was drawn more than a quadrant from its quiescent position, and motion communicated to the chronometer.

The effect of the balance on a pocket compass was observed in another experiment; and a table is given in the paper, illustrating the deviations produced in it, by moving the balance through given arcs. An arc of 110° produced a deviation of 51½°. A compass needle of a more delicate construction was inverted, the moment the time-screws had passed through an arc of 90°. A curious effect was also remarked by Mr. Harvey, by turning the balance through a greater arc than a quadrant, and thereby causing the north pole of the compass to point west, when, by allowing the balance to oscillate, the compass needle ranged for many seconds through the com-

plete circumference, until the directive power of the earth, by gaining the ascendancy, caused the arcs of vibration successively to diminish; the needle ultimately obtaining a position coincident with the meridian, where it continued in a state of tremulous motion as before.

Mr. Harvey remarks, that the quantity of steel contained in the chronometer was truly remarkable, and no part of it was destitute of vigorous polarity. Every screw displayed its influence, and of which there were ten large, and several small ones, in the frame alone. The chain also, the axles of the different wheels and pinions, the arbor of the fusee, the balance and its spring, exhibited the same intense and active power. Nor did this polarity partake of the transient character of that imparted by induction from the earth to soft iron, but was permanent, undergoing no sensible alteration from change of position.

8. *Influence of Magnetism on the Rates of Chronometers.*—This interesting and curious subject continues to interest philosophers, and Mr. Harvey, in the XIXth and XXth Numbers of the *Edinburgh Philosophical Journal*, has two papers, devoted to the consideration of the changes which time-keepers undergo, altering their positions with respect to the attracting force.

A pocket chronometer, having a very steady and uniform rate of $+20''.4$, was placed with its main spring nearly in contact with the magnet, and with the magnetic power directed through its centre, when the rate altered to $+65''.1$; but on moving the centre of the main spring 90° from the preceding position, so as to cause the magnetic power to be transmitted through the centre of the balance, the rate immediately declined to $-23'.2$; and on turning the timekeeper another quadrant, so as to remove the centre of the mainspring 180° degrees from its first situation, the rate again rose to $+43''.4$; and when through another quadrant, the attractive force being in this situation transmitted nearly through the centre of the balance, the rate became $-2''.6$; and on restoring it to its first position $+72''.7$. When the time-keeper was detached, its rate returned to $+18''.2$. Similar experiments with another chronometer, having a detached rate of $-2''.0$, produced in situations corresponding to the last, the rates $+10''.0$, $+3''.1$; $+5''.0$, and $-1''.1$. From these experiments, Mr. Harvey deduces, that *an increase of rate resulted from the direct transmission of the magnetic influence through the centre of the main spring; and a diminution thereof, when the same power passed nearly through the middle of the balance and its spring.*

Mr Harvey has, however, not only determined the effect of the direct transmission of the magnetic power, through the centre of the main-spring, but also that produced by its partial operation. For this purpose, the first of the before-mentioned chronometers was so

placed, that a radial line proceeding from the centre of the time-keeper through the middle of the main-spring, might form an angle of 27° with the longitudinal axis of the magnet. The consequence of this application was an immediate increase of +20″.1, its detached rate, to +52″.3; a quantity *less* than the mean of the two results obtained from the *direct* transmission of the magnetic power through the centre of the spring, by +16″.6. By pursuing this branch of the subject, the author of the experiments found, that the removal of the centre of the spring from the axis of the magnet, through equal arcs, appeared to produce proportional declensions of rate. In one experiment, the rates +68.″9 and +43.″4, produced by the direct transmission of the attractive force through the centre of the main-spring, and when this point was at its *least* and *greatest* distance from the pole of the magnet, are very nearly proportional to +50″.8, and +33″.7, the rates obtained, when the radial line proceeding from the centre of the time-keeper through the middle of the main-spring, formed respectively angles of 27° and 153°.

An exception to the above conclusions was discovered by Mr. Harvey, when experimenting with another chronometer, the accelerations in the rate having taken place when the magnetic power was transmitted through the centre of the balance; and the retardations, when it passed through the middle of the main-spring; and the author, when alluding to this anomalous result, properly observes, in the pursuit of experimental science, every result ought to be fairly and impartially recorded. The admirable maxim of Bacon, *we cannot control Nature, unless by making her manifest*, should ever be present to the mind of the inquirer.

The influence also of magnetized plates is illustrated by several experiments. Two chronometers, when placed on a circular magnetic plate, *lost* by having XII turned from N. to E.; *gained* by being turned from E. to S.; *lost* from S. to W.; and *gained* from W. to N.; the changes from *plus* to *minus* being alternate. It was found also, that the difference even of one-eighth of an inch, in the position of the chronometer on the magnetized plate, was constantly accompanied by a sensible alteration of rate. The rate was always augmented by moving it nearer to the north pole; and the most considerable alterations were found in the east and west positions of the time-keeper, when the line drawn from the axis of the chronometer to the centre of the balance, was at right angles to the meridian of the magnetized plate. The smallest changes were also produced in those situations of the chronometer corresponding to north and south, the centre of the balance being in those positions of the machine, in the magnetic axis of the plate.

9. ***On the Adaptation of a Compound Microscope, to act as a Dynameter for Telescopes.*** By C. R. Goring, M.D.—It appears to me

that at this moment a simple, cheap, and accurate dynameter, is rather a desideratum; the best, I believe, now in use is that invented by the late ingenious Ramsden, whose ruling passion seems to have been not only to surmount difficulties, but to create them also in many instances. He seems to have selected one of the most complicated and difficult principles to carry into effect on which a dynameter can be formed; and however excellent it may be in itself, very few workmen of the present day will undertake to execute dynameters of his construction. In consequence the most common instrument of the kind is nothing more than a mother-of-pearl micrometer, with divisions of an inch into 200 parts, attached to a lens. This again is too coarse an instrument, and is, moreover, very difficult to use, having no contrivance to adjust it to perfect vision on the pencil of light, in addition to which it frequently cannot be adapted to measure high powers at all, from an impossibility of getting it close enough to the eye-piece, the brass work of which will not permit the plate of the micrometer to arrive at the point on which a very short pencil of rays falls. To obviate all these inconveniences, nothing more would be necessary than to use a compound microscope, having the micrometer at its field-bar, in the focus of the eye-glass. It will be very easy to shew that this sort of dynameter will be perfectly commodious, not liable to get out of order, and susceptible of any degree of accuracy which we may think it necessary to obtain; I am only surprised that it is not to be found in all the opticians' shops.

Let us suppose the object-glass of such a microscope to be $\frac{1}{4}$ inch focus, that the eye-glass is 1 inch focus, with a negative field-glass, and that there is a micrometer of mother-of-pearl at the field-bar having divisions to the $\frac{1}{300}$ of an inch, (which I know by experience can be read with a lens of 1 inch focus).—According to the length of the tube of the microscope the image at the field-bar will be more or less magnified,—say it is 7 times larger than the object—then a pencil of rays of $\frac{1}{500}$ of an inch diameter will subtend $\frac{7}{500}$ on the micrometer, and may be seen divided into 7 parts, therefore it may be measured to the $\frac{1}{3500}$ of an inch, a degree of accuracy quite sufficient, I apprehend, for practical purposes—if not, we have only to increase the depth of the object-glass, and we may obtain a scale to any extent we please. In the same manner, if the divisions of the micrometer are not seen with sufficient ease, the depth of the eye-glass may be augmented. Were it an object to carry this principle to its utmost extent, one of Mr. Troughton's micrometers might be attached to the body of the microscope; but this I apprehend would be quite superfluous.

One circumstance in constructing this dynameter must be strictly attended to; I mean the ascertainment of the exact value of the divisions of the mother-of-pearl, which is done with perfect facility by viewing another similar micrometer placed in the focus of the

object-glass; and by shortening or lengthening the tube of the microscope, the divisions may be made to coincide in any point which is selected. I think it will be found convenient to have the micrometer in the field-bar on a narrow slip of mother-of-pearl divided into 100 parts to one inch, and then again into 5 more, and to adjust the eye-glasses and the length of the tube so that $\frac{1}{10}$ of an inch in the focus of the object-glass shall be equal to 1 inch at the field-bar, and so to fill the whole of the field of view. It would perhaps facilitate the reading of the divisions if a dot were placed at every tenth of an inch on the micrometer.

With respect to mechanical arrangements, the body of the dynameter should be made to slide up and down in another tube with or without rack work, which may be pressed firm by the hand against the eye-piece of the telescope, whose powers it is applied to measure; while the internal microscope is adjusted to distinct vision, the external tube may be easily made applicable to any telescope, or a shoulder with a screw might be left upon every eye-piece to which the said tube may be firmly attached. It will be evident that the object-glass of such a dynameter will always be at an abundant distance from the shortest pencil of rays it is employed to measure.

I should scarcely have thought it worth while to have pointed out so obvious an application of the compound microscope, but I have never seen or heard of its adaptation to any such purpose as I have recommended. It must be recollected that some have the faculty of perceiving things for themselves, others only when they are pointed out to them, and many hardly then;—of this the history of Columbus and his egg will remind us.

II. Chemical Science.

1. *On a Reciprocity of insulating and conducting Action which the incandescent Platina of Davy exerts on the two Electricities.*—The following is part of an extract communicated to the *Annales de Chimie*, (xxv. 278.) from a memoir of M. Erman, inserted in the memoirs of the Academy of Berlin, for the years 1818 and 1819.

Place on an electrometer an aphlogistic lamp, of which the upper spirals of platina wire are in full incandescence, and hold at the distance of four or five inches above the lamp the negative pole of a dry voltaic pile, or the negative coating of a small Leyden jar feebly charged, the electrometer will diverge powerfully. Present in the same manner the positive pole or coating, there will be no divergence or at least a very slight one, and that due to induction.

Place above an insulated aphlogistic lamp, at the distance of four or six inches, a small screen of any conducting substance, making it communicate with an electrometer, then touch the lamp with a positive pole or coating, and the electrometer of the screen will diverge

powerfully; but if the lamp be touched with a negative pole or coating, no divergence, or only a very slight one, will take place.

The following table will illustrate the difference of effect when the lamp was postive and negative. The first column is the number of inches between the lamp and the screen above.

	Lamp positive.	Lamp negative.
1 inch	The leaves opened to their full extent (14 lines) in 1″ and discharged themselves against the side every second.	The leaves opened the 14 lines in 345″
2 ,,	Ditto in 1½″	345″
3 ,,	,, 2″	540″
4 ,,	,, 3″	leaves diverged one line in 150″ and only diverged 2½ lines on the whole.
5 ,,	,, 3½″	1 line in 210″ total divergence 1½ lines.
6 ,,	,, 4½″	1 line in 240″ total divergence 1 line.

A similar, but inverted table would represent the progress of the electrometer attached to the lamp, the screen being similarly electrified.

There exists, therefore, incontestibly a reciprocity of conducting and insulating actions; the lamp conducts and transmits the positive effect to the screen, but not the negative; on the contrary, the screen transmits the negative effect to the lamp, but not the positive. This singular property is found to exist in all the combinations of this kind which can be imagined. Thus, for example, if a Leyden jar is moderately charged positive by its ball, and this applied to the insulated aphlogistic lamp, a smaller Leyden jar, with its ball held about four or six inches from the incandescent platina, will become very sensibly charged; but if the ball of the first be electrized negatively, there will be no charge given to the second, on repeating the experiment. By disposing successively a number of electrometers, each with its aphlogistic lamp, so as to establish a communication from one to the other, a very paradoxical system is obtained, representing a species of pile which is rapidly traversed by positive electricity from right to left, but not at all in the opposite direction, whilst with negative electricity the inverse directions are equally distinct: though as the author has not succeeded in increasing the effect by the successive groups, it has perhaps more analogy with the tourmaline. Electro-magnetic phenomena were not known at the time when M. Erman discovered the reciprocity of insulating and conducting action, and he has not as yet published the result of his ultimate researches on the electro-magnetic effects of incandescent platina.

It may, perhaps, be supposed that the effect is due to the power the ascending current of vapours has to take positive electricity with it to the screen above, and probably the experiment may be considered as a proof of the truth of Franklin's theory, and an argument against Dufay. But it is not in a vertical direction only that the aphlogistic lamp receives negative electricity from without, and not positive; but in all directions, and from all the concavity of a sphere, of which the lamp is the centre. It is not therefore on an emanation in the direction of the ascending current of vapours that the effect depends, but it resembles rather a radiation like that of light and heat. Secondly, the reciprocal effect essentially requires the actual incandescence of the upper spirals of platina; without this the apparatus may be disposed so as to emit a much larger quantity of vapours, but in vain. Thus, for instance, 397 grains of platina, which arranged properly on a wick would light amadou at two lines distance, and keep 400 grains of water boiling, offered nothing like the reciprocal action because the upper spires were not ignited; whilst a spiral only a few grains in weight, but incandescent to its extremity, acted in a most decided manner. Thirdly, heated iron offers some traces of this reciprocity, but only whilst it is in full ignition. The effect cannot, therefore, depend on a heated current, which would continue long after ignition had ceased. M. Erman has also seen many cases in which the iron has had the opposite power to the platina, emitting the negative and receiving the positive electricity. Finally, as an argument against the efficacy of a heated current, undecomposed vapour at a high temperature possesses no power of conducting electricity.

Without insisting much upon it, M. Erman suggests the following explanation of the phenomena:

There exists two electricities, between which there is a specific difference of expansibility: the heat of incandescence acts by augmenting this expansibility, in the same manner as the pointed form of conductors augments the tension. If this augmentation of expansibility be very considerable, the specific difference of the two electricities disappears in the greatness of the whole effect: this is the case with flame; but there exists a certain degree of heat which augments the expansibility in a less degree, and precisely to the point at which the most expansible of the two (the positive) is able to overcome the constraining force of the circumambient medium, whilst the less expansible (the negative) notwithstanding the increment of force it has received, has not yet attained to the point at which it can overcome the resistance of the medium. The action of the incandescent wire is, therefore, according to this view, connected with the phenomena of the specifically different lights, presented by points positively or negatively electrified. Sufficient examination has not been made whether points, not incandescent, emit different quantities of electricity according as they are positive or negative; but the marked

effects of a pile of a single metal, terminating at one side in a point, and the other in a large surface, and placed end to end in water, with merely this geometrical difference, proves evidently that something of this kind exists, and it was whilst occupied with these piles on geometrical principles that the author was conducted to researches on incandescent points.

If the rays of the sun, by heating the soil, produce an effect analogous to that spoken of, *i. e.*, to increase the electric repulsion, but only in the proportion required to make the positive fluid overcome the resistance of the air, and not the negative, it would explain the habitually positive state of the lower strata of the atmosphere. The author has not, however, found this idea confirmed by experiments with the aphlogistic lamp. In fact, when left on a condenser for several hours it had not disturbed the electrical equilibrium, *i. e.*, the excess of expansibility acquired by the positive electricity was not sufficient to detach it from its combination with the negative electricity.

2. *On the Magnetic Action of strong electrical Currents on different Bodies.*—Coulomb, in 1802, gave the results of a well-known series of experiments on the action exerted by the opposite poles of two powerful magnets on minute needles of any substance delicately suspended between them. It was found that, whatever the nature of the substance, the needle ultimately arranged itself in the direction of the poles; but he finally concluded that this was due to the minute portions of iron which they contained.

M. Biot, who repeated these experiments very carefully, is not entirely of this opinion; but suggests, that inasmuch as simple contact of heterogeneous bodies is sufficient to develop electrical forces, which for a long time were quite unsuspected, perhaps other circumstances may develop similar or analogous forces extremely feeble, but sufficient to affect apparatus delicate as Coulomb's.

After this M. Ampere, with M. A. Delarive, made an experiment at Geneva on the effect of electrical currents on a plate of copper, and conceived that the copperplate, by being near the currents, was capable of affecting the magnet like the neighbouring wires, through which the current was passing, but afterwards ascertained that this was not the case.

Ultimately M. Becquerel has resumed the examination of these or similar phenomena, making use of Schweigger's multiplier for the concentration of the powers of the electrical current, and he has observed differences between the effects thus produced and those obtained in M. Coulomb's experiments. The galvanometer used was 1.97 inches long and about 0.4 inches wide. Care was taken that the substances should not be worked with iron instruments, and the needles formed of them were made very small, especially if of a substance but feebly affected by the electrical current; they were then suspended

in the galvanometer just as a magnetic needle would be, and a Wollaston's pile of 10 pair of plates connected with the wires of the instrument.

A needle of soft iron instantly placed itself parallel to the axis of the spirals, the arrangement of magnetism in it being similar to that of a common bar magnet.

Deutoxide of iron enclosed in a small paper cartridge 0.157 of inch in diameter, and properly suspended, was rapidly drawn into the plane of the apparatus, and took a position perpendicular to the axis of the spirals; but soft iron filings similarly circumstanced acted just like the iron needle.

The difference exhibited in this way between these two substances does not exist in Coulomb's manner of making the experiment, and hence a difference of action would seem to be indicated between the electro-magnetic wire and magnetic poles.

Needles of copper, wood, or gum lac, were affected like the deutoxide of iron, but in a smaller degree; but great caution is requisite in making these experiments to avoid currents of air; this is best done by closing the extremities of the galvanometer in glass.

Needles thus affected by the electric current were then examined as to their action on a feeble magnet. The iron needle was found to act like a regular magnet, and it is to be presumed that the cartridge of iron filings did so also; but the parcel of deutoxide of iron, when examined, was found to act with one pole of the bar in the same manner at every point situated towards one side of the galvanometer, and inversely when the pole was changed, so that the north magnetism was on one side of the needle, and the south on the other. It is, however, possible to distribute the magnetism as in the common needle, which is done by retaining the cartridge for some time parallel to the axis of the instrument; but when left to itself, it returns gradually to the state described. The action of the magnet on the other needles, when in the galvanometer, gave no certain results.

A needle of wood about 1 inch in length, and .04 of an inch in diameter, had fixed at each extremity a square plate of steel or soft iron .08 of an inch in the side, and .008 of an inch in thickness, when placed in the spiral it was rapidly drawn into a position parallel to the plane of the spirals, the distribution being as in the cartridge of deutoxide. Two pieces of iron wire 0.04 of an inch in length were then put in place of the plates, and now the needle stood in the instrument at an angle of 45° with the plane of the spirals, or with its axis; as the length of the ends of iron wire was increased the needle tended more to parallelism with the axis, and when these extremities were 0.4 of an inch long, the needle stood parallel to the axis of the instrument.—*Ann. de Chim.* xxv. 269.

3. *On Electro-motive Actions produced by the contact of Metals and*

Liquids, &c., by M. Becquerel.—The apparatus used by M. Becquerel to collect and indicate the electricity developed by the contact of a solid with a liquid is a condensing electroscope of extreme sensibility, invented some time since by M. Bohnenberger*, but varied and rendered more delicate for the present purpose by M. Becquerel. The instrument of the latter philosopher consisted of a single dry voltaic column fixed horizontally on a wooden support, and having attached to each of its poles, in a vertical position, a plate of metal about 3 inches long: these plates are placed near together, and a slip of gold leaf hung between them, connected with a condensing plate 9 inches in diameter. The sensibility of this apparatus is such that a tube of glass rubbed on cloth acts in dry weather at a distance of 8 or 10 feet, and the electric state of the hand or hair has an influence at the distance of several feet. Hence the utmost precaution is requisite in experimenting with the instrument.

The following are some of the experiments made with this apparatus, the gold leaf being in communication in all of them with the lower plate of the condenser. A brass capsule containing an alkaline solution, or ammonia, was placed on the upper plate of the condenser: a communication was then made with the solution by touching it with the finger, or a moistened band of cloth, and the lower plate was also connected with the earth; a few moments after the upper plate was raised, and the gold leaf moved towards the positive pole; thus the alkaline solution, or ammonia, by contact with the copper, had taken positive electricity, and the metal negative electricity.

When sulphuric acid was used in place of alkali, opposite electrical effects were produced, the acid became negative, and the metal positive.

A platina capsule filled with an alkaline solution was placed on the upper condensing plate; the under plate was then touched on the one hand by a plate of platina, and on the other the liquid was touched by the finger, and in this manner the electro-motive actions of the platina on the copper was neutralized, being the same on both sides, and therefore the upper plate would only retain the electricity due to the contact of the platina with the solution. Sometimes it is necessary to put a small piece of paper between the platina and the copper, for the apparatus is so sensible, that a very small difference in the state of the surfaces is sufficient to influence the results. Operating thus the same results were obtained as before with alkali; the platina became negative, and with acid it became positive. A zinc capsule filled with solution of soda became negative, and with concentrated sulphuric acid positive; when the acid was diluted no electricity was developed. Silver became very feebly electrical in contact either with acid or alkali.

* See vol. xi. p. 208, of this Journal.

In general metal in contact with acid becomes positive, making the acid negative, and with alkalies the reverse effects are obtained; but there are many cases, as with silver, in which the electromotive action can scarcely be observed.

"Sir H. Davy had found that those acid and alkaline substances which can exist in a dry and solid form become electrical by their contact with metals; thus perfectly dry oxalic or succinic acid, either in powder or mass, when placed on a copperplate, takes negative electricity, and communicates positive electricity to the metal. The celebrated English chemist found also, that in consequence of the difficulty of depriving potash and soda of water, they did not in general produce any electricity by their contact, but that after being strongly calcined they possessed for a moment the power of becoming electric by contact with a metal. He endeavoured also to determine, by means of very delicate instruments, the electric state of an insulated acid or alkaline solution after their contact with the metal, but there were no results of that nature."

"We have proved, therefore, that the electric effects observed by Sir H. Davy as produced by the contact of a solid dry acid or alkali with a metal, and where consequently there is no chemical action extends to the contact of all the metals with acid or alkaline solutions, even though sometimes chemical action may have commenced."

Having determined the electric state of an acid or alkaline solution in contact with a metal, the next object was to ascertain the effect when the solution was placed between two different metals. The copper capsule was placed on the upper condensing plate, and filled with an alkaline solution or very dilute sulphuric acid; the solution was then touched by a plate of zinc, taking care that the two metals did not come in contact, and the lower condensing plate was touched with the finger; twenty seconds after, on raising the upper plate the gold leaf moved towards the positive pole, so that the copper capsule had become positively electrified. On putting a zinc capsule on the upper plate with one of the two solutions, touching the lower plate with a piece of zinc so as to neutralize the electromotive action of that metal on the copper, and touching the liquid with a piece of copper held in the hand, an electrical state was produced, which, on raising the upper condensing plate, made the gold leaf move towards the negative pole, consequently the zinc capsule had become negative. Hence it is seen, that when zinc and copper are separated by an acid or alkaline solution, the zinc becomes negative, and the copper positive, which is the inverse of that which takes place when the two metals are in contact.

Another result obtained experimentally by M. Becquerel is, that copper in a solution of muriate of soda becomes negative, and the solution positive.—*Ann. de Chim.* xxv. 405.

4. *Measurement of the conductibility of Bodies for Electricity.*—M. Rousseau has, for several years past, been occupied in the construction and observation of dry voltaic piles, and has lately applied them to the determination of the conducting power of bodies, in regard to electricity. MM. Ampere and Dulong were directed by the French Academy to report on a memoire by M. Rousseau on this subject, and the following statement is drawn up from that report. The dry pile is formed of discs of zinc-leaf, and tinsel, separated by discs of parchment, previously imbibed with a mixture of equal parts of oil of poppies and oil of turpentine; the whole pile is covered with resin to prevent the contact of the air. The pole is fixed vertically communicating below with the earth, the upper end is made to communicate at pleasure by a wire to a pivot, on which is placed a weakly-magnetized steel needle, and also to a metallic ball placed at the same height as the needle, and not quite half its length from the pivot; hence, when the communication is made the needle and ball are similarly electrified and the needle is repelled; and when the needle and ball are previously placed in the magnetic meridian, the position to which the needle is repelled is proportionate to the magnetic and electric forces, and is constant for a very considerable time for the same apparatus. The magnetic needle might be replaced by a simple electric needle suspended by a wire of proper length and diameter, forming a balance of torsion; but the arrangement of M. Rousseau is more convenient, and sufficiently sensible.

On using the instrument, the substance of which the conductibility is to be measured, is made part of the connexion between the top of the pile and the needle and ball, care being taken that the portion traversed by the electricity is always of the same dimensions. If the time occupied in producing the greatest deviation is not instantaneous, then the period which passes before the needle takes a permanent position is a measure of the conductibility of the substance employed.

Liquids, when tried, are put into small metallic vessels communicating with the ball and needle, then a wire partly covered with gum lac, except for a certain length at the extremity, has that uncovered portion entirely immersed in the fluid, so that the same surface is always in contact; then, on connecting the other end of the wire with the end of the pile, the time which passes before the needle is at its maximum deviation is observed, and is inversely as the conducting power of the liquid.

Observing in this manner, a remarkable fact was noticed with olive oil, for, notwithstanding its similarity to other oils, it was found to be exceedingly inferior in conducting power. Thus all other things being equal, olive-oil required 40′ to produce a deviation produced in 27″ by poppy and other oils, and on adding to olive-oil only $\frac{1}{100}$ of another kind of oil, the time was reduced to 10′. Hence any adulteration of olive-oil is easily discoverable by the instrument.

Solid fat conducts with less facility than animal oils, from the ex-

cess of stearine in it, for it was ascertained that elaine conducted electricity much better than stearine. The fat of an animal diminishes in conducting power with the age of the animal. The same apparatus also marks a notable difference between resin, gum lac, and sulphur, the most insulating of all bodies known, and also between silk, flint-glass, and common glass.

As to alcoholic or aqueous fluids, acids, alkalies, *&c.*, the time required was too short to be adopted as a measure, but a modification of the apparatus would enable it to measure the conducting power of all of them. It is remarked also in the report, that "it would be equally possible and very curious to make trial of the two electricities on various substances, for it would be sufficient for that purpose to put the poles of the pile alternately in communication with the earth. It is probable, according to the results formerly obtained by Erman, that differences would be found with certain substances.—*Ann. de Chim.* xxv. 373.

5. *Distinction of Positive and Negative Electricity.*—Positive and negative electricity may be readily distinguished by the taste, on making the electric current pass by means of a point on to the tongue. The taste of the positive electricity is acid, that of the negative electricity is more caustic and, as it were, alkaline.—*Berzelius.*

6. *Electricity produced by Congelation of Water.*—When water is frozen rapidly in a Leyden jar, the outside coating not being insulated the jar receives a feeble electrical charge, the inside being positive, the outside negative. If this ice be rapidly thawed, an inverse result is obtained, the interior becomes negative, and the outside positive.—*Grothus.*

7. *Hare's Single Gold-leaf Electrometer.*—This instrument consists of a glass vessel, fixed by a foot on to a wooden stand, and having an aperture at the top and also another at one side. The top is closed by a metal cap, finished externally by a horizontal zinc disc, six inches in diameter, and connected internally with a single leaf of gold cut into an acute triangular form, and hanging in the centre of the instrument with the point downward. Opposite to the lower end of this leaf of gold is a ball attached to a horizontal wire, and which passing through a screw cap fixed in the lateral opening of the glass vessel, can be made to approach to, or recede from, the leaf at pleasure, the distance being estimated by a graduation on the screw into $\frac{1}{100}$th parts of an inch. A plate of copper six inches in diameter, and furnished with a glass handle, generally accompanies the instrument.

"The electricity produced by the contact of copper and zinc is rendered sensible in the following manner: Place the disc of copper on the disc of zinc, take the micrometer-screw in one hand, touch

the copper disc with the other, and then lift this disc from the zinc. As soon as the separation is effected the gold leaf will strike the ball, usually if the one be not more than $\frac{3}{100}$ of an inch apart from the other." "That the phenomenon arises from the dissimilarity of the metals is easily shewn by repeating the experiment with a zinc disc, in lieu of a disc of copper. The separation of the homogeneous discs will not be found to produce any contact between the leaf and the ball."

"It is probable that the sensibility of this instrument is dependant on that property of electricity which causes any surcharge of it which may be created in a conducting surface, to seek an exit at the most projecting termination or point connected with the surface," this disposition being increased of course by the proximity of the ball. These effects are not to be expected in weather unfavourable to electricity, but in favourable circumstances they have been produced by a smaller instrument, the discs being only two inches and a half in diameter.

8. *Hare's Voltaic Trough.*—Dr. Hare states that, having had occasion to remark the surprising increase in the deflagrating power of a series of galvanic pairs, when, after due repose, they were simultaneously exposed to the acid, he was induced to devise means of accomplishing this object in various ways, and that ultimately the following method occurred to him as the best: Two troughs are joined lengthwise edge to edge, so that when the sides of the one are vertical, those of the other are horizontal. Then by a partial revolution of the two troughs, thus united upon pivots which support them at the ends, any fluid which may be in one trough must flow into the other, and on reversing the motion, must flow back again. The galvanic series being placed in one of the troughs, the acid in the other, by a movement such as above described, the plates may all be instantaneously subjected to the acid, or relieved from it.

The pivots are made of iron, coated with brass or copper, as less liable to oxidizement; they are connected within with the galvanic series, and move on pieces of sheet-copper, which are easily made the extremities of connecting pieces, and thus the whole can be arranged in any way found convenient.

9. *Dobereiner's Instantaneous Light Apparatus.*—Since the very curious observation made by M. Dobereiner of the power possessed by spongy platina of determining the combination of oxygen and hydrogen at common temperature, that substance has been applied, among other uses, to the construction of an instantaneous light apparatus; a jet of hydrogen is thrown on to a portion of the spongy platinum, and is by it inflamed. Various modes of presenting the plantinum to the hydrogen have been devised, but none surpass or even equal that originally adopted by M. Dobereiner. The extremity of a fine platina wire is to be rolled into a spiral form, and then

dipped into ammonio-muriate, or muriate of platina, until about two grains are taken up, after which it is to be heated red-hot in a spirit lamp. In this way a quantity of spongy platina is formed on the wire so minute, that if put into contact with a mixture of oxygen and hydrogen it becomes heated, and inflames the gas as rapidly almost as if an electrical spark had passed. Such a wire as this fixed on the jet-pipe, so that the spongy metal shall be exposed to the current of hydrogen, immediately inflames it. It happens that if an instrument of this kind has been exposed for some hours to a humid atmosphere, the inflammation does not take place readily, but in this case if the top of the platina be touched by the finger or palm of the hand, either before or during the time that the current of hydrogen is passing out, the inflammation immediately takes place. Contact, indeed, is not necessary, for the mere approach of the hand is sufficient to elevate the temperature so much as to cause instant inflammation.

In using spongy platina for eudiometrical purposes *, M. Dobereiner attaches his balls to the end of a platina wire, so as to be able to withdraw them when the experiment is completed, or even during the experiment if requisite, so that they may be dried and again introduced.—*Bib. Univ.* xxv. 117.

10. *Test of the Alteration of Solutions by contact with Air.*—M. Becquerel remarks, that if iron be dissolved in nitric acid, and the solution filtered, and two plates of platina connected with the two extremities of the wire of a galvanoscope, be immersed into the solution, and if one plate be withdrawn, and then re-introduced into the solution, it will produce an electric current passing from this plate to the other; and generally the plate withdrawn from the solution and re-introduced becomes positively electrical.

The nitrates of copper and lead give similar results, but they do not retain this power, and in the course of a few hours no effects of this kind are observable. Nitrate of zinc does not operate in this manner. Suspecting that the effect was due to the action of air on the film of solution which adheres to the withdrawn plate, the experiment was made in an atmosphere of hydrogen, and then no such results were obtained. M. Becquerel, therefore, attributes the effect to the alteration induced by the air on the portion of solution withdrawn with the plate, and which, when the plate is re-immersed, being dissimilar to the fluid that has not been exposed, determines the current of electricity. The effect of the air he considers is probably to convert such portion of deutoxide of azote and proto-nitrate as may have been formed by the action of nitric acid on the metal into nitrous acid and deuto-nitrate, and that when this has taken place

* See Vol xvi. page 374.

with all the portions of the solution the power of producing electrical currents ceases.—*Ann de Chim.* xxv. 413.

11. *Odour of Hydrogen Gas extraneous, Inodorous Hydrogen Gas.*—When hydrogen gas, obtained from a mixture of iron filings and diluted sulphuric acid, is passed through pure alcohol, the hydrogen loses its odour in a great measure; and if water be added to the alcohol it becomes milky; if enclosed in a flask and left for some days, an odorous volatile oil is deposited, which was contained in the gas, and which contributed to its well known odour.

Perfectly inodorous hydrogen gas may be obtained by putting an amalgam of potassium and mercury into pure distilled water, but if an acid or muriate of ammonia be added to the water, which accelerates the development of gas, it gives it the same odour as that remarked in the solution of zinc by weak sulphuric acid. This odour, therefore, does not belong to the hydrogen gas, but is given to it by impurities.—*Berzelius.*

12. *Inflammation of Sulphuretted Hydrogen by Nitric Acid.*—When a few drops of fuming nitric acid are put into a flask filled with sulphuretted hydrogen, the hydrogen is oxidized by the nitric acid, and the sulphur is disengaged in a solid form. If the flask be closed with the finger, so that the gas which becomes heated cannot escape, its temperature is raised so much as to produce combustion with a beautiful flame, and a slight detonation which forces the finger from the mouth of the flask. This experiment may be made without the least danger, with a flask containing four or five cubical inches of gas.—*Berzelius.*

13. *Artificial Chalybeate Water.*—If a few pieces of silver coin, (says Dr. Hare,) be alternated with pieces of sheet iron, on placing the pile in water it soon acquires a chalybeate taste and a yellowish hue, and in twenty-four hours flocks of oxide of iron appear. Hence by replenishing with water a vessel, in which such a pile is placed, after each draught we may obtain a competent substitute for a chalybeate spring.

14. *Mercurial Vapour in the Barometer.*—M. Billiet observes, that "for a long time past it has been known that during hot seasons mercurial vapour has formed spontaneously in the upper part of the barometer tube, which condenses in minute drops on its inner surface. It is sufficient for the observation of this phenomenon at pleasure to apply a small tin vessel, filled with ice, to this part of the tube for an hour or two. On removing the cooling vessel there may be perceived on the internal surface of the tube a dimness about six lines in diameter, and by means of a lens it will be found that this is nothing but a

mass of minute globules of mercury attached to the glass, those in the centre being largest. Hence arises the question, whether this vapour may not have some influence on the oscillations of the barometer?—*Bib. Univ.* xxv. 93.

15. *Combustion of Iron by Sulphur.*—Dr. Hare makes this experiment in the following manner:—A gun-barrel is heated red at the butt end, and a piece of sulphur thrown into it; then either blowing through the barrel, or closing the mouth with a cork, will produce a jet of sulphurous vapour at the touch-hole, to which if iron wire be exposed it will burn as if ignited in oxygen gas, and fall in fused globules of proto-sulphuret of iron.

16. *Ammonia in Oxides of Iron.*—M. Chevalier has stated to the Royal Academy, that he has ascertained the presence of ammonia in various oxides of iron, and promises further accounts.—*Ann. de Chim.* xxv. 429.

17. *Iodous Acid.*—Il Sig. Sementini, of Naples, has published an account of a combination of iodine and oxygen, containing less of the latter principle than iodic acid. It is obtained in the following manner:—equal parts of chlorate of potassa and iodine are to be triturated together, in a glass or porcelane mortar, until they form a very fine pulverulent yellow mass, in which the metallic aspect of the iodine has entirely disappeared. If there be excess of iodine the mixture will have a lead colour. This mixture is to be put into a retort the neck being preserved clean, and a receiver is to be attached with a tube passing to the pneumatic trough. Heat is then to be applied, and for this purpose a spirit lamp will be found sufficient; at first a few violet vapours rise, but as soon as the chlorate begins to lose oxygen dense yellow fumes will appear, which will be condensed in the neck of the retort into a yellow liquid, and run in drops into the receiver; oxygen gas will at the same time come over. When the vapour ceases to rise, the process is finished, and the iodous acid obtained will have the following properties:—

Its colour is yellow; its taste acid and astringent, and leaving a burning sensation on the tongue. It is of an oily consistency, and flows with difficulty. It is heavier than water, sinking in it. It has a particular odour, disagreeable, and something resembling that of euchlorine. It permanently reddens vegetable blues, but does not destroy them as chloric acid does. It is very soluble in water and alcohol, producing amber-coloured solutions. It evaporates slowly, and entirely in the air. At 112° F. it volatilizes rapidly, forming the dense vapour before mentioned. It is decomposed by sulphur, disengaging a little heat, and liberating violet vapours. Carbon has no action on it at any temperature. Solution of sulphurous acid decomposes it as well as iodic acid, precipitating the iodine as a brown

powder. It is characterized by the manner in which potassium and phosphorus act on it: the instant they touch it they inflame; the potassium producing a white flame and dense vapours, but little or no liberation of iodine; and the phosphorus, with a noise as of ebullition, violent vapours appearing at the same time.

The odorous nature of this acid, its volatility, colour, and its power of inflaming phosphorus by mere contact, shew that some of the principal characters of iodine are retained, and that it is oxygenated, therefore, in a minor degree, and deserves the name of iodous acid.

Its composition has not been experimentally ascertained. M. Sementini endeavoured to analyze it by putting 100 grains into the end of a long sealed tube, and then dropping a small piece of phosphorus in, iodine was disengaged, and condensed in the upper part of the tube, and this was found to amount to 45 grains: but this can furnish only very uncertain results.

Iodous acid dissolves iodine, becoming of a deep colour, more dense and tenacious, and having more strongly the odour of iodine. when heated the iodine partially rises from the iodous acid, but they cannot be separated in this way.

M. Sementini believes also in an oxide of iodine, and has given the name to the black powder, which is produced by the action of sulphurous acid on iodous acid, and which still contains oxygen, but he mentions that this and some other points still require investigation.

The following are the properties of the iodic and iodous acids, by which a judgment may be formed of their specific difference. *Iodic acid* is solid, white, without odour, reddening blue colours, and then destroying them. Volatile at 456° F., with decomposition heated with charcoal or sulphur it is decomposed with detonation. *Iodous acid* is liquid, yellow, odorous, reddening blue colours, but not destroying them; volatilizing at 112° F., and even at common temperatures without decomposition; heated with sulphur it is decomposed without detonation, and inflames potassium and phosphorus by mere contact.—*Bib. Univ* xxv. 119.

18. *Preparation of pure Oxide of Uranium.*—The following is M. Arfwedson's mode of procuring oxide of uranium pure. Finely pulverized pechblende is to be dissolved by a gentle heat in nitro-muriatic acid, after which a good deal of water is to be added, and a little muriatic acid, if necessary. The undissolved matters, consisting of sulphur, silica, and a portion of the gangue, are to be removed, and a current of sulphuretted hydrogen passed through the solution as long as it affects it. The first precipitate is dark coloured, but the latter portions being sulphuret of arsenic is yellow. On filtration, the liquor is free from copper, lead, and arsenic, but contains iron, cobalt, and zinc. It is now to be digested with a little nitric acid to

peroxydize the iron, and then decomposed by carbonate of ammonia, in excess, which leaves the iron and earths; the filtered solution is to be boiled as long as carbonate of ammonia is disengaged, the oxides of uranium, zinc, and part of the oxide of cobalt falls down, and is to be collected on a filter, washed and dried. It is then to be heated to redness, by which it becomes of a dark green colour, and afterwards by maceration in dilute muriatic acid has the oxides of cobalt and zinc, with a small portion of oxide of uranium, dissolved out, and after washing and drying, pure oxide of uranium remains. About 65 per cent. of the pechblende used was obtained in this way.

19. *Uranium Pyrophori.*—When solutions of per-nitrate of uranium and nitrate of lead are mixed together, and precipitated by caustic ammonia, a precipitate falls, which M. Asfwedson considers as an uraniate of lead; after being washed, heated, and pulverized, it was of a cinnamon brown colour. This substance being placed in a tube was heated, and hydrogen gas passed over it, much water was formed, and it is to be presumed that the lead and the uranium were both reduced to the metallic state. The product was a dark brown powder, which when exposed to the air on paper, took fire and ignited, leaving uraniate of lead as a residue. This singular phenomenon, which was quite unexpected, may have been occasioned (M. Arfwedson suggests) by an electro-chemical action between the two metals, which caused their combustion.

When uraniate of barytes is reduced by hydrogen in a similar manner it also produced a body presenting the same phenomenon in the air; and the pyrophorus thus obtained from the uraniate of iron is still more powerful than either of the former.

20. *Atomic or proportional Weights.*—Dr. Thomson gives the following as the most correct expression of the atomic weights of the substances mentioned according to his last experiments:

Boracic acid	3.00
Tartaric acid	8.25
Fluoric acid	1.25
Fluoboric acid	4.25
Tartaric acid crystallized .	9.375
Oxygen being	1.00

The crystals of tartaric acid contain 1 proportional of water.—*Ann. Phil. N. S.* VII. 245.

21. *On the Acetates of Copper.* By M. Vauquelin.—The following results are abstracted from a paper by M. Vauquelin, read to the Academy of Sciences, Nov. 6, 1823, and published in the *Mémoires du Museum*, x. 295.

Analysis of the Crystallized Acetate of Copper.—A given weight was

pulverized, mixed with nitric acid in a porcelain crucible, and heated ultimately to redness, it yielded 40 per cent. of black oxide of copper. Other portions were heated to a temperature sufficient to dissipate the water, but not to decompose the salt. In these cases the loss was never more than 10 per cent., and was very constant. In respect to the acid, two grammes of the salt were dissolved in four grammes of sub-carbonate of potash; the mixture filtered, all the soluble portions collected, and after being carefully neutralised by sulphuric acid, evaporated to dryness, and digested in alcohol. This solution again evaporated gave 1.8 grammes of acetate of potash, which containing 0.93 of a gramme of acetic acid, gives a proportion of 46.5 per cent. on the acetate of copper employed. The atomic composition of this salt is therefore given as nearly the following:—

Acetic acid	2 atoms	=	12.7 5	or per cent	51
Oxide of copper	1 ,,	=	10	,,	40
Water	3 ,,	=	2.25	,,	9

When solution of crystallized acetate of copper is boiled for some time it is decomposed, a little acetic acid escapes, much black oxide of copper falls down, and when the decomposition ceases, which it always ultimately does, another acetate of copper is found in the solution. This decomposition takes place in close vessels, where no acetic acid is allowed to escape. One hundred parts of the crystalline acetate deposit about 14.65 of oxide, leaving in solution 25.35 parts in combination, with twice its weight of acetic acid.

On continuing to boil the solution, no further deposition of oxide took place; as concentration proceeded acetic acid escaped, but sufficient remained to keep all the oxide in solution. Ultimately the usual crystallized acetate was obtained, which when dissolved in water and boiled, precipitated oxide as before, so that by several operations the whole might be decomposed in this manner.

Verdigris is known to be a mixture of the crystallized acetate of copper, and a sub-acetate. A portion of the latter was extracted by washing pulverized verdigris rapidly, with successive small portions of cold water, to avoid a decomposition afterwards to be noticed; this, when dried, was analyzed in a manner somewhat like the preceding, and found to consist of nearly 66.5 oxide, and 33.5 acid. Hence there are three combinations of acetic acid, and oxide of copper, containing, the first, 66.5, the second, 44.44, and the third 33.34 of oxide, supposing them all dry.

M. Vauquelin remarked also a singular decomposition of verdigris which takes place spontaneously, and without the assistance of heat. If 1 of verdigris be mixed with 500 of distilled water, and left at a temperature of 60° or 70° F. it gradually becomes yellow, then brown, and in seven or eight days no green portions are observed. When filtered, per-oxide is obtained, and a blue solution, which

when boiled becomes turbid, and deposits more oxide. Although the quantity of water is mentioned above, yet the decomposition takes place with other proportions, but most rapidly when the proportion is greatest; 100 parts of verdigris were found to leave about 23 parts of oxide of copper. In order to ascertain the correctness of an opinion, that it was the sub-acetate only in the verdigris which underwent this change, some of that salt was prepared, and one part mixed with 500 of water, and agitated from time to time. At first the salt swelled and became flocculent, then it became yellow, and at last brown, diminishing rapidly in volume. These effects were more rapid in the sun's rays, without doubt from the heat produced. The per-oxide, when collected gave 46 per cent. of the sub-acetate employed, just double that afforded when verdigris was used, and the soluble crystallized acetate of copper formed remained in the solution, as was proved by boiling the solution; it underwent a further decomposition, just as the crystalline acetate had done before.

"Thus," says M. Vauquelin, "there are three combinations of the oxide of copper and acetic acid: 1st, a sub-acetate insoluble in water, but decomposing in that fluid, at common temperatures becoming per-oxide, and an acetate; 2nd, a neutral acetate, the solution of which is not altered at common temperatures, but is decomposed by ebullition, changing into per-oxide, and a super-acetate; 3rd, a super-acetate, which, when in solution is not decomposed, either at common temperatures, or at the point of ebullition, and which cannot be obtained crystallized, except by slow spontaneous evaporation, or evaporation in a vacuum."

22. *Dahline or Inuline in the Jerusalem Artichoke.*—M. Braconnot, whilst engaged lately in an examination of the tubercles of the Helianthus Tuberosus, or Jerusalem Artichoke, ascertained the presence of a substance in them, in all respects resembling the Dahline of M. Payen. The recent tubercles were rasped, pressed, and the juice collected; left to itself it deposited a substance like starch, which, when collected and boiled in water, was almost entirely dissolved; but on evaporation a substance was deposited like the Dahline. (See vol. xvi. p. 387.) M. Braconnot, however, does not think that this, or M. Payen's substance should be considered as a new proximate principle, but considers them both as specimens of Inuline.—*Ann. de Chim.* xxv. 361.

23. *New Vegeto-alkalies.*—*Violine.*—At a sitting of the Académie Royale de Médicine, M. Boullay read a memoir on the analysis of the violet, *viola odorata*, from which it appears that the violet contains an active alkaline, bitter and acrid principle, similar to the Emetine of Ipecacuanha, and which is called by the author, *Emetine of the violet, indigenous emetine*, or *violine.* According to M. Orfila it

possesses powerful poisonous qualities. It was found to reside equally in the root, leaves, flowers, and seeds of the plant; but associated with different proximate principles, so as to have its action on the animal system modified.—*Jour. de Pharmacie, Jan.* 1824.

24. *Jalapine* or *Jalapia.*—Mr. Hume, jun. of Long Acre, is said to have discovered a vegeto-alkaline principle in Jalap, and proposes to call it *Jalapine.* It is procured in the following manner. Coarsely powdered jalap is macerated for 12 or 14 days, in strong acetic acid; a highly coloured tincture is thus obtained, which, when filtered, is to be supersaturated with ammonia, and thmic violently shaken: a sabulous deposit will fall rapidly, and a few crystals will form on the sides of the vessel. The deposit and crystals are to be collected and washed with distilled water, again dissolved in a small quantity of concentrated acetic acid, and re-precipitated by ammonia added in excess, which throws down the jalapine in small white acicular crystals.

Jalapine is without any perceptible taste or smell, and seems to be heavier than Morphia, Quinia, or other substances of this class; it is scarcely soluble in cold water, and only to a small extent in hot water; ether has no effect upon it; alcohol is its proper solvent. Very little trouble is requisite to purify jalapine from extractive or colouring matter, for which it appears to have but a slight affinity.

Mr. Hume has not made many experiments upon this substance, but thinks that one ounce of jalap will, on careful treatment, afford about five grains of the substance.—*Med. Jour.* li. 346.

25. *MM. Liebeg and Gay Lussac on Fulminic Acid and Fulminates.* An abstract was given in the last number of this journal, (p. 153.) of a paper by Dr. Liebeg on fulminating silver, mercury, *&c.*, in which the author proved that they were saline compounds containing a peculiar acid, which he called the fulminic acid, and the compounds of this acid with bases he called fulminates; shewing at the same time that they all possessed similar properties to the compounds of silver and mercury. Since the researches referred to, Dr. Liebeg has been joined by M. Gay Lussac in further investigations on this subject, and the remarkable result has been obtained that cyanic acid is the true acid existing in these compounds. The paper containing these ultimate investigations is published in the *Annales de Chimie,* xxv. 285. and contains admirable examples of chemical reasoning and manipulation; but we cannot do more at present than give a very brief account of it.

The compound principally experimented on was that of silver; the fulminate of silver from its insolubility being more readily obtained perfectly pure than any other. It was prepared by putting 6.5

grains of nitric acid,' s. g. 1.36 or 1.38 into a pint matrass, and a piece of coin, containing nearly 35 grains of pure silver. The resulting solution was poured into about 927 grains of strong alcohol, and heated until it boiled; on the appearance of turbidness it was removed from the fire, and an equal quantity of alcohol added by degrees to cool the solution and moderate the ebullition. When cold, the whole was filtered, and the precipitate washed with pure water until no longer acid. It is then perfectly pure, and white as snow. The filter was put on a plate which was placed on a saucepan half filled with water, and heated to 212°, for two or three hours, that it might be perfectly dry; its weight was generally equal to that of the silver employed.

Fulminate of silver will not detonate alone at 212° F. or even at 266° F. but a slight blow between two hard bodies, even under water, will explode it: hence wooden stirrers and paper spoons should be used in experiments made with it.

When mixed with 40 times its weight of per-oxide of copper it may be rubbed in a porcelain capsule with the finger, or a cork, and does not then detonate by heat. This mode of analysis was therefore adopted to ascertain the proportion of carbon and nitrogen in the salt, or rather in the acid. The gaseous mixture obtained by heat contained exactly 2 volumes of carbonic acid, and 1 volume of nitrogen: hence, these elements are in the same proportion as in cyanogen.

Fulminate of silver contains two proportions of oxide, one belonging apparently to the acid, and the other, serving as base; Muriatic acid; entirely decomposes the fulminate, giving a chloride equivalent to the oxide contained in the metal; operating in this way, 100 of the compound gave as a mean result, 77.528 of oxide of silver, or

Silver	. .	72.187
Oxygen	.	5.341

It is assumed that the silver is all in the state of oxide, a supposition supported by all the results.

Muriate of potash precipitates only the silver serving as base, and does not affect that of the fulminic acid; and operating with it instead of muriatic acid, 100 of the compound gave a quantity of chloride equivalent to 38.105 of oxide of silver; and the solution remaining, which contained the fulminic acid united to potash, when decomposed by muriatic acid yielded chloride equivalent to 38.359 oxide of silver. Hence it may be concluded that the fulminate of silver contains twice as much oxide of silver as will saturate fulminic acid.

When the compound was decomposed by oxide of copper and heat, a process which was conducted with the utmost attention and accuracy, 100 of fulminate of silver gave a mean of carbon and nitrogen, equivalent to 17.16 of cyanogen; small quantities of water were obtained, but they were irregular, and never amounted to any thing like a proportional of hydrogen in the compound. Other proofs

were also obtained during the investigation of the absence of hydrogen.

Thus far, then, the elements obtained from the fulminate of silver were:

Silver . . .	72.187
Oxygen . .	5.341
Cyanogen . .	17.160
Loss . . .	5.312
	100.000

The loss is very nearly equal to the quantity of oxygen combined with the silver, it could not be hydrogen or water, neither of which could have escaped the search made by the experimentalists, it could only therefore be oxygen contained in the fulminic acid, a supposition ultimately confirmed. Fulminate of silver, therefore, contains

2 atoms of silver,
2 atoms of oxygen combined with the silver,
2 atoms of oxygen combined with the elements of fulminic acid,

$$2 \text{ atoms of cyanogen,} = \begin{cases} 2 \text{ atoms nitrogen,} \\ 4 \text{ atoms carbon.} \end{cases}$$

It was desirable, if possible, to ascertain the products of the detonation of this substance, but after some trials the danger made it necessary to desist. Endeavours were then made to decompose it by heat, when previously mixed with substances that could not furnish oxygen. Glass in impalpable powder always exploded it, but chloride of potassium and sulphate of potash fused and finely pulverized could be mixed with it by the finger or a cork without producing explosion. The chloride of potassium gave inconvenient and uncertain results, in consequence of its partial decomposition by the silver, and the production of carbonate of potash, as well also as of carbonate of ammonia. When the sulphate of potash was used, the gaseous and other results furnished by heating the fulminate were first collected, examined, and ascertained, and then the residue in the tube was mixed with oxide of copper, and heated for the decomposition of the substance operated on. A quantity of nitric acid was produced in the latter part of the operation, and at times also carbonate of ammonia in minute quantity; in the latter case no water could be perceived, and it appeared that the formation of one of these compounds excluded that of the other; for it was found by direct experiment that when the fulminate was first slightly moistened much carbonate of ammonia was formed: thus then a new proof was obtained of the absence of hydrogen from the compound, for the quantity of carbonate of ammonia was so small, it could not have resulted from any proportional quantity of hydrogen in union

with the other elements, but only from a minute trace of water introduced with the materials operated upon.

From further experiments of this kind it was concluded, that when fulminate of silver mixed with sulphate of potash was decomposed by heat, only half its carbon became carbonic acid, and only that proportion of nitrogen was set free which with the carbon would form cyanogen, so that the silver was left in a state of a subcyanuret.

If the elements thus analytically obtained are correct, the following will be the equivalent number of fulminic acid:

1	atom oxide of silver . .	145.161
2	— cyanogen . .	65.584
2	— oxygen	20.000
		230.745

and on experiment it was found that 3.833 of fulminate of baryta decomposed by muriatic acid gave 1.585 of chloride of barium, which by calculation would give 228.873 as the number of fulminic acid, a result sufficiently in accordance with the former to justify the calculated number.

The authors then consider the probable nature of fulminic acid. That the metal should be an essential principle can hardly be imagined, inasmuch as one metal may be replaced by another; thus a fulminate may be obtained with zinc only, analogous to that of silver: are not therefore the various fulminic acids formed by the different metals super-salts, of which the acid really contains no metal but only cyanogen and oxygen?

As fulminates may be obtained with oxides which lose their oxygen with difficulty, oxide of zinc for instance, as well as with silver or mercury, it is evident they must all include one common principle of fulmination independent of the bases, and which can only be a compound of oxygen and cyanogen, or of oxygen, carbon, and nitrogen. Again, if the fulminates be compared to neutral tartrates, and the various fulminic acids to bitartrate, a perfect analogy will be found; thus neutral tartrates of zinc, copper, silver, or mercury, are only half decomposed by potash, just like the fulminates of the same bases: all the fulminic acids form double salts with bases like the bitartrates: fulminic acid with a base of silver is, in consequence of its insolubility, precipitated by acids like cream of tartar: and there are many fulminates, as well as neutral tartrates, in which acids produce no precipitates, because the corresponding acid fulminates, or tartrates, are soluble; such are the fulminates and tartrates of zinc and copper. Hence it appears to the authors extremely probable, if not certain, that the various fulminates form a particular class of salts, all containing the same acid composed of an atom of cyanogen and an atom of oxygen only, and which is without doubt the cyanic acid. The neutral fulminates

are *cyanates*, and the various fulminic acids *bi-cyanates*, and the equivalent number of cyanic acid will be 42.792, oxygen being 10.

All attempts to separate the acid from the fulminates failed. Muriatic acid, hydriodic acid, and sulphuretted hydrogen, decompose the fulminate of silver even at common temperatures, giving rise to some particular results, which are described at considerable length in the Memoire.

For the preparation of alkaline fulminates, it is recommended that the chlorides should be used: thus, to obtain the double fulminate of silver and potash decompose the fulminate of silver by solution of chloride of potassium, being careful to add no more of the chloride than is sufficient to precipitate half the silver, or even a smaller quantity; for the undecomposed fulminate of silver being scarcely soluble, will remain with the chloride of silver, and the solution will contain the pure double fulminate of silver and potash. Cautions are again enforced at the end of this paper on the care required in working with these substances.

26. *Supposed new Metal, Taschium.*—A description of a new metal, with an accompanying specimen, has been sent to the President of the Royal Society.

The metal has received the name of Taschium, from the mine of Taschio, in which it was found.

The specimen sent was said to be silver containing the new metal, the two metals having been separated by amalgamation, and the mercury afterwards driven off. On dissolving the button in pure nitric acid, it was stated that the Taschium would remain as a black powder.

The Taschium was described as being combustible, with a bluish flame, a peculiar smell, and dissipation of the products. Amalgamating with mercury, and in that way being separated from its ores. Not soluble in any single acid, but soluble in nitro-muriatic acid. If previously boiled with potash, then soluble in muriatic acid, the solution being precipitated by water. Its solution giving, with prussiate of potash, a blue precipitate brighter even than that with solution of iron, but not precipitating with tincture of galls.

The button was therefore dissolved in nitric acid, which left a blackish powder in small quantity, and also some grains of siliceous sand. The powder was well washed, and then being heated on platina foil in the flame of a spirit lamp, did not burn or volatilize, but became of a deep red colour. Muriatic acid being added to another portion of the washed powder, and a gentle heat applied, dissolved by far the greater part of it, forming a red solution, which being evaporated till the excess of acid was driven off, and then tested, gave blue precipitate with prussiate of potash; black with tincture of galls; and reddish-brown with ammonia. On evaporating to dryness, it left muriate of iron. Nitro-muriatic acid being made to act on the minute portion of powder yet remaining, dis-

solved very nearly the whole of it, leaving a small trace of silica, and producing a solution similar to the former. Hence the Taschium in this button of silver was nothing else than iron; and from the presence of silicious sand it may be supposed to have been introduced into the button through the inaccuracy of the preparatory manipulations.—M. F.

27. *Liquefaction of Sulphurous Acid.*—In the *Annales de Chimie et Physique* for May last, M. Bussy is stated to have obtained the above acid liquid, and free from water, by causing it to pass in its gaseous state through a tube containing fused chloride of calcium, and afterwards into a flask surrounded by a mixture of ice and salt, where it completely liquifies, and remains in a liquid state under atmospheric pressure at the temperature of 0°. It is a colourless, transparent, and very volatile liquid, of a specific gravity = 1.45. It boils at about 10° centigrade below 0 = 14° Fahrenheit, but in consequence of the cold produced by the evaporation of the portion which is volatilized the residue remains liquid, being reduced to a temperature much below its boiling point. It occasions intense cold, and rapidly evaporates when dropped upon the hand. Poured into water at common temperatures one portion is dissolved and another volatilized; but as the solution approaches to saturation, the acid collects in drops at the bottom of the vessel, like an oil heavier than water. If in this state it be touched by the extremity of a glass tube, it passes into vapour, occasioning ebullition, and ice forms upon the surface of the water.

The bulb of a thermometer enveloped in cotton, and dipped into the liquid acid, falls spontaneously, when exposed to the air, to − 57°. (= − 70° Fahr.) The atmosphere being at 50° F. In the vacuum of the air-pump a cold of −68° (= −90° F.) is thus easily obtained*. Mercury therefore is easily frozen by the aid of this acid, simply by dipping the bulb of a mercurial thermometer surrounded with cotton into it, and agitating the air with it. The experiment succeeds better when a little mercury is put into a cup with a small quantity of sulphurous acid upon it, and the whole put under the exhausted receiver. By the evaporation of the acid in vacuo, M. Bussy *has frozen alcohol of a strength below* 33° (of a specific gravity below .852 at 55°). By passing chlorine and ammonia through tubes cooled by the evaporation of sulphurous acid, M. B. liquefied those gases; and by a similar method cyanogen was obtained in the form of a crystallized solid.

* M. Bussy says these low temperatures can only be accurately measured by an air thermometer.

III. Natural History.

1. *On the different Manners in which Bodies act on the Organs of Taste*, by M. Chevreul.—Persuaded as I am that many phenomena appear complicated to us only because they are the results of many causes acting simultaneously, I have adopted as a principle, when I examine phenomena of this kind, to endeavour to separate the different causes which may operate so as to refer to each the effects dependant on it. Viewing from this point the varied sensations which we perceive when substances are introduced into the mouth, I have arrived at a satisfactory analysis of these sensations in recognising those which are perceived; 1st. By the touch of the tongue; 2d, by the taste; 3d, by the smell. It is generally known that we can perceive these three orders of modifications by the introduction of substances into the mouth; but since no physiologist that I have consulted has indicated the means of recognising the special modifications belonging to the senses of touch, taste, and smell, I have determined to publish the following results, which make part of my *general considerations on immediate organic analysis, and on the application of this branch of chemistry to the history of organized beings.*

It is not possible to separate the action which a substance introduced into the mouth exerts on the touch from that exerted by it on the taste, but it is easy to distinguish the effects produced on each of these senses; for that purpose one must first appreciate the effect produced by the substance on the organ of touch by applying it to some other part of the body than the tongue, and then this effect may mentally be abstracted from that produced when the substance is put into the mouth, and by this means the effect produced on the taste will be obtained, except that as the tongue is more sensible than the skin, the sensation of touch on the tongue will be stronger than that on the skin elsewhere. For instance, if a little powdered chloride of lime be pressed upon the skin the water of transpiration will be solidified by the compound, and a sensation of heat experienced. If, on the contrary, crystallized muriate of lime in powder be used, it will liquefy, and a cold sensation be felt. It is evident therefore that chloride of lime put into the mouth will produce heat, whilst the muriate of lime will produce cold, and that these effects will be more marked than on the surface of the body, since the tongue is more sensible and more humid than the skin. The substances which fusing or evaporating on the surface of the body produce cold, will also produce the same effects in the mouth if they fuse or evaporate there.

But how are the sensations of smell to be separated from those of the touch and taste? Very simply; pressing the two nostrils one against the other is sufficient to prevent all sensation of smell, because

then the air, which becomes more or less charged with the odorous particles which a sapid and odoriferous substance in the mouth has emitted not being able to pass by the nose, cannot any longer carry those particles to the membrane which occasion the sensation of smell. When therefore the nostrils are pressed together, no other sensations are perceived than the taste and the touch of the tongue. One can hardly form an idea of the extreme difference which exists between the sensations produced by a sapid and odorous substance in the mouth according as the passage of the air expired by the nose is open or interrupted.

I have ultimately established four classes of bodies relative to the sensations which they excite when put into the mouth, amongst which I do not include those caustic substances which attack and alter the organs.

1st Class. *Bodies which act on the tongue only by touch.*—Rock-crystal, sapphire, and ice.

2d Class. *Bodies which act by touch on the tongue and by smell.*—The odorous metals: when tin is put into the mouth the odour of that metal is perceived; but on pressing the nostrils all sensation, except that of touch only, entirely disappears.

3d Class. *Bodies which act by touch on the tongue and by taste.*—Such bodies as these are sugar, salt, *&c.* When these substances are put into the mouth the sensations they cause are not modified by pressing the nostrils together.

4th Class. *Bodies which act by touch on the tongue, and by taste and smell.* Examples 1. *Volatile Oils.*—They are generally acrid, but with a particular odour for each sort of oil. When put into the mouth, and the nostrils are pressed, the acrid sensation is always sensible, whilst that of smell vanishes entirely. 2. *Lozenges of Peppermint, Chocolate, &c.*—When the nostrils are pressed, after these have been introduced into the mouth, nothing is perceived but the savour of the sugar; but if the nostrils be relieved, the odour of the peppermint or the chocolate becomes evident.

It will not be useless to remark that the urinous taste attributed to fixed alkaline bases, does not belong to these substances, but to the ammonia, which is set at liberty by their action on the ammoniacal salts contained in the saliva. The proofs of this are the disappearance of the sensation referred to, when the nostrils are pressed, and the perception of the same sensation when one smells to a mixture of recent saliva and alkali, made in a small glass or porcelain capsule.

It appears that the sense of smell weakens by age, before that of taste.—*Mem. du Mus.* x. 439.

2. *Action of Meconic Acid on the Animal Economy.*—Doubts having arisen with regard to the effects produced by pure meconic acid and the meconiates on the animal system, i Signori Fenoglio, Cesare,

and Blengini, of Turin, prepared some of these substances very carefully, and administered them in cases where the results could be accurately observed. It was found that eight grains of any of these substances produced no deleterious effects on dogs, crows, or frogs; nor on a horse even when the dose was repeated. The meconiates were also administered to two persons in cases of tænia, in doses of four grains, but without producing any effect either on the persons or the worms. These results agree with those obtained by MM. Suertuerner and Sœmmering: and in those cases where death was produced by doses of a grain of meconic acid, Dr. Fenoglio attributes the results to the defective preparation of the substance, and the presence of morphia in it; and the symptoms observed seem to accord with this opinion.

3. *On the different masses of Iron which have been found on the Eastern Cordbiliera of the Andes.* By MM. de Rivero and Boussingault.—On arriving at Santa Rosa, a village situated on the road from Pamplona to Bogota, we learnt that a mine of iron had been discovered in the neighbourhood, and that a fragment of the mineral served for an anvil to a farrier (or blacksmith); but we were agreeably surprised when we saw that this supposed mineral was a mass of iron full of cavities, of an irregular form, and presenting all the characters of meteoric iron.

This mass was found on the hill of Tocavita, about a quarter of a league to the east of the village in 1810. We went to the place, and saw the hole from whence the mass had been removed, for it was almost entirely under-ground, a point of a few inches only appearing at the surface. The hill of Tocavita, like that of Santa Rosa, belongs to the secondary sandstone formation, and which we have observed for a considerable extent.

Santa Rosa is about twenty leagues N.E. of Bogota, lat. 5° 40′, long. 75° 40′ west of Paris, and 2744 metres (9003 feet) above the sea. The people of the village collected together to remove the mass of iron: it remained eight years at the town-hall, and afterwards for seven years did service in the blacksmith's shop.

The iron was cellular, but no vitreous coat could be perceived on it. It was malleable, of a granular structure, easily gave way to the file, was of a silvery aspect, and its specific gravity 7.3. The volume of the mass was 102 cubic decimetres (3.6 cubical feet), its weight therefore must be nearly 750 kilogrammes (1655 lbs.)

It is worthy of observation, that at the same time that this mass was discovered, a number of smaller fragments were found on different parts of the same hill. During the short time which we remained in the place, we collected several specimens. To demonstrate the identity of these masses with those which various travellers have described, some chemical examinations were undertaken. The usual

process of analyses is [then described. A portion of the large mass yielded

Oxide of iron .	1.17	or	Iron .	91.41
Oxide of nickel	0.15		Nickel .	8.59
				100.00

Some of the other fragments were then examined. "We commenced with a mass weighing 681 grammes (10,517 gr.) discovered in 1810, near Santa Rosa. It was malleable, but difficult to file. Its lustre was silvery; its grain fine like that of steel; it forged very well but was red short; its specific gravity 7.6; it gave,

Oxide of iron .	9.46		Iron	91.23
Oxide of nickel	0.75	or	Nickel	8.21
Insoluble in nitric acid	0.02		Residuum	0.28
				99.72

The insoluble portion was difficultly acted upon by hot nitro-muriatic acid; it appeared to be a compound of nickel, iron, and perhaps a little chromium.

Another fragment of 561 gram. (8664 gr.) found at the same time near Santa Rosa, was cellular, very hard to the file, but malleable, of a silvery aspect, and a fracture resembling that of tilted cast-steel; it gave,

Oxide of iron .	2.62	or	Iron	91.76
Oxide of nickel .	0.16		Nickel	6.36
				98.12

We have also ascertained the presence of nickel in a great number of other fragments, collected at the same time near Santa Rosa, the weight of the largest being 145 grammes (2229 grs.) But it is not there only that metallic iron has been found; it has also been discovered at a village called Rasgata, in the neighbourhood of the salt-works of Zipaguira, lat. 4° 57′, long. 76° 33′ west of Paris, and 2650 metres (8694 feet) above the level of the sea. We saw one mass in the hands of M. Geronimo Torres weighing 41 kilogrammes (lbs.) We could perceive no cavity in it; its texture exhibited small facets; it was very hard to the file, was malleable, of a silvery lustre, and a specific gravity of 7.6; it gave,

Oxide of iron .	5.23	or	Iron .	90.76
Oxide of nickel .	0.40		Nickel .	7.87
				98.63

Another mass weighing 22 kilogrammes (90.5 lbs.), which was shewn us at the same place, was nearly spherical, and contained many cavities. It was very malleable, and its fracture had a silvery lustre. We found from seven to eight per cent. of nickel in it.—*Ann. de Chim.* xxv. 438.

4. *Natural Ice Caves.*—In a memoir on some natural ice caves, read

by Professor Pictet, to the Helvetic Society, in 1822, the author had advanced the singular fact, attested by the neighbouring inhabitants, that *the ice forms more in summer than in winter*, and conceived that this effect might be due to two concomitant causes; descending currents of air, and the cold produced by evaporation.

It was desirable that this fact should be confirmed by observation made in the winter; a season, however, when the fall of snow prevented ascents to any great height. One of these natural ice caves visited by Professor Pictet, is situated near the crest of the Mont Vergy, in Faucigny; it is called from the name of the neighbouring chalet, Montarguis. Two countrymen of the village of Sionzier, near the road to this ice-cave, had the curiosity and perseverance to make three visits to this place during the last autumn and winter, and have drawn up a short notice, which has been read to the Geneva Society. It is as follows:

"The 22d Oct. we ascended to the ice-cave of Montarguis with some little trouble, because of the first snow, and we found very little ice in columns; it had begun to melt.

"The 26th November we re-ascended to the before-mentioned ice-cave. There we found very little ice at the bottom of the cave, out of which came a sort of warmth.

"The 25th Dec. we re-ascended to the above-mentioned cave with much difficulty and trouble, and were almost carried away by an avalanche. This circumstance discouraged us, but recovering from our fear we ascended. There we found a moderate warmth in the cave, and no ice; instead of which where there is ice in summer, there was actually water: therefore in winter it is warm in this cavern, and in summer it is cold. The roof appears cavernous; it appears as if there were chimneys."

The fact, therefore, seems well ascertained, and the editor of the *Bibliothéque Universelle* observes, that the concluding remark comes in support of the explanation given by Professor Pictet, depending on descending currents of air, cooled by evaporation, whilst traversing considerable strata of stones constantly moist. This effect can only take place in summer, for in winter the current of air would be ascending from the superior warmth of the interior to the exterior.

The descending current of cold air was observed during the last summer by M. Gampert, who visited this cave, and penetrated to its extremity; there he discovered a crevice, or aperture, by which water descended and flowed over the ice, and also a very rapid current of very cold air.—*Bib. Univ.* xxv. 243.

5. *Glacier of Getros, Valley of Bagne.*—The glacier of Getros, in the valley of Bagne, has been noticed at different times in this journal*, and the ingenious and successful means adopted by M. Venetz for its

* See Vols. v. p. 372. vi. 166. xv. 396.

destruction described. This means had, at the end of the summer of 1822, reduced the glacier, which originally covered the river for a length of 1380 feet, to an extent of 498 feet only. The cold-winter of 1822-3 and the following spring, increased the glacier to 924 feet, and this new part was excessively rugged and dangerous to work upon, and continually exposed to masses falling from the upper glacier. It was requisite, however, that this should be first destroyed, which was done at the risk of many serious accidents by currents of water as before, during the summer of 1823, and such advantage taken of the rest of that cold short summer as to diminish the whole glacier to 252 feet only. Thus, notwithstanding the accessions which it must have received during the last winter, there is little doubt but that it will be entirely removed during the present summer, and then the course of the river being open, it will generally remove all the avalanches that may fall at any future period; or if a disastrous year like that of 1816 gives rise to the formation of a new glacier, the means for its removal are known, and may be practised before the formation of another lake can again destroy the country.

ART. XV.—METEOROLOGICAL DIARY for the Months of March, April and May, 1824, kept at EARL SPENCER'S Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For March, 1824.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn	Eve.
Monday	1	31	43	29.62	29.53	NW	W
Tuesday	2	28	35	29.51	29.51	NW	NbW
Wednesday	3	29	36	28.88	29.10	WbN	N
Thursday	4	25	39	29.68	29.65	NW	WbS
Friday	5	32	49	29.37	29.47	WbS	WbN
Saturday	6	36	51	29.60	29.60	WbS	SW
Sunday	7	45	59	29.26	29.37	SSW	SW
Monday	8	34	51	28.94	28.98	S	SW
Tuesday	9	34	46	29.50	29.49	SW	SW
Wednesday	10	35	40	29.52	29.70	NE	NE
Thursday	11	28	40	29.87	29.53	W	SW
Friday	12	32	45	29.40	29.32	W	NW
Saturday	13	29	44	29.24	29.18	W	NW
Sunday	14	32	43	29.69	29.85	N	NW
Monday	15	29	45	29.85	29.78	S	S
Tuesday	16	47	51	29.76	29.76	SW	WbS
Wednesday	17	37	52	29.90	30.00	W	W
Thursday	18	39	58	30.02	30.02	WbS	W
Friday	19	43	53	30.08	30.10	W	W
Saturday	20	34	54	30.10	30.05	E	SbW
Sunday	21	44	48	29.75	29.67	S	WNW
Monday	22	33	37	29.53	29.50	WbS	NW
Tuesday	23	34	44	29.64	29.77	NE	ESE
Wednesday	24	35	41.5	29.85	29.87	NNE	NE
Thursday	25	37	43	29.90	29.98	NE	NE
Friday	26	38	46	29.90	29.88	NE	NE
Saturday	27	36	44	29.75	29.68	NW	NE
Sunday	28	34	41.5	29.78	29.80	NE	NE
Monday	29	6.5	45	29.83	29.73	WbN.	W
Tuesday	30	37	47	29.61	29.54	W	N
Wednesday	31	39	40	29.58	29.61	NNW	NNW

For April, 1824.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve	Morn.	Eve
Thursday	1	25.5	47	29.75	29.60	NW	W
Friday	2	35	44	29.00	29.55	WNW	NW
Saturday	3	29	51	29.84	29.92	NW	NW
Sunday	4	28	50	30.10	30.14	NE	E
Monday	5	25.5	49	30.29	30.30	ENE	ENE
Tuesday	6	31.5	47.5	30.30	30.30	NE	NE
Wednesday	7	36	46	30.17	30.10	NW	NE
Thursday	8	38	50	30.14	30.00	N	NE
Friday	9	40	50.5	30.00	29.80	NbW	WNW
Saturday	10	35.5	44	29.39	29.39	NbW	NNW
Sunday	11	31	43	29.30	29.30	NW	NW
Monday	12	33	47	29.30	29.41	NW	W
Tuesday	13	31	50	29.58	29.67	W	W
Wednesday	14	30	57	29.70	29.77	W	NW
Thursday	15	28.5	55	29.75	29.60	E	E
Friday	16	36	45	29.40	29.30	ENE	ENE
Saturday	17	39	50	29.50	29.70	ENE	NE
Sunday	18	35	54	29.80	30.10	NE	E
Monday	19	32	57	30.20	30.17	ESE	SE
Tuesday	20	37	60	30.18	30.16	SE	SW
Wednesday	21	35	62	30.00	29.79	SE	SE
Thursday	22	47	64	29.77	29.80	SW	SW
Friday	23	47	54	29.38	29.40	SE	NW
Saturday	24	45	58	29.82	30.00	NW	WbN
Sunday	25	43	59	29.98	29.80	SW	SE
Monday	26	48	56	29.60	29.40	S	WbN
Tuesday	27	44	62	29.58	29.70	W	SW
Wednesday	28	51	58	29.70	29.70	SW	SbE
Thursday	29	51	64	29.60	36.52	SW	SE
Friday	30	50	66	29.40	29.53	S	WbS

For May, 1824.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Saturday	1	50	61	29.72	29.80	NbW	W
Sunday	2	47	62	29.80	29.74	NbE	NE
Monday	3	46	48	[illegible]	29.62	NW	NW
Tuesday	4	41	56	29.44	[illegible]	W	W
Wednesday	5	40	63	29.68	29.70	SW	W
Thursday	6	42	66	29.70	29.70	W	[illegible]
Friday	7	47	67	29.70	29.84	WbS	W
Saturday	8	50	65	30.00	30.11	W	NE
Sunday	9	48	63	30.17	30.10	E	SE
Monday	10	42	69	29.97	29.66	ESE	ESE
Tuesday	11	45	55	29.84	29.73	E	EbN
Wednesday	12	42	52	29.82	29.80	EbN	NE
Thursday	13	39	49	29.77	29.68	NE	NE
Friday	14	39	43	29.52	29.59	E	NNE
Saturday	15	41	44	29.45	29.46	NNE	NE
Sunday	16	42	53	29.48	29.73	NE	NE
Monday	17	33	60	29.87	29.80	WbN	W
Tuesday	18	45	58	29.80	29.72	WbN	W
Wednesday	19	43	57	29.63	29.68	NE	WbN
Thursday	20	39	59	29.60	29.60	W	NW
Friday	21	30	61	29.74	29.78	NW	NW
Saturday	22	34	60	29.84	29.84	NE	NE
Sunday	23	37	61	29.80	29.78	WNW	NW
Monday	24	47	69	29.78	29.82	NW	NE
Tuesday	25	45	69.5	29.98	30.03	ENE	NE
Wednesday	26	48	68	30.30	30.36	NE	W
Thursday	27	45	72	30.36	30.45	WbN	E
Friday	28	43	75	30.31	30.30	SE	SW
Saturday	29	43	68	30.19	30.00	WSW	E
Sunday	30	49	60	29.75	29.70	E	SE
Monday	31	50	69	29.70	29.75	W	W

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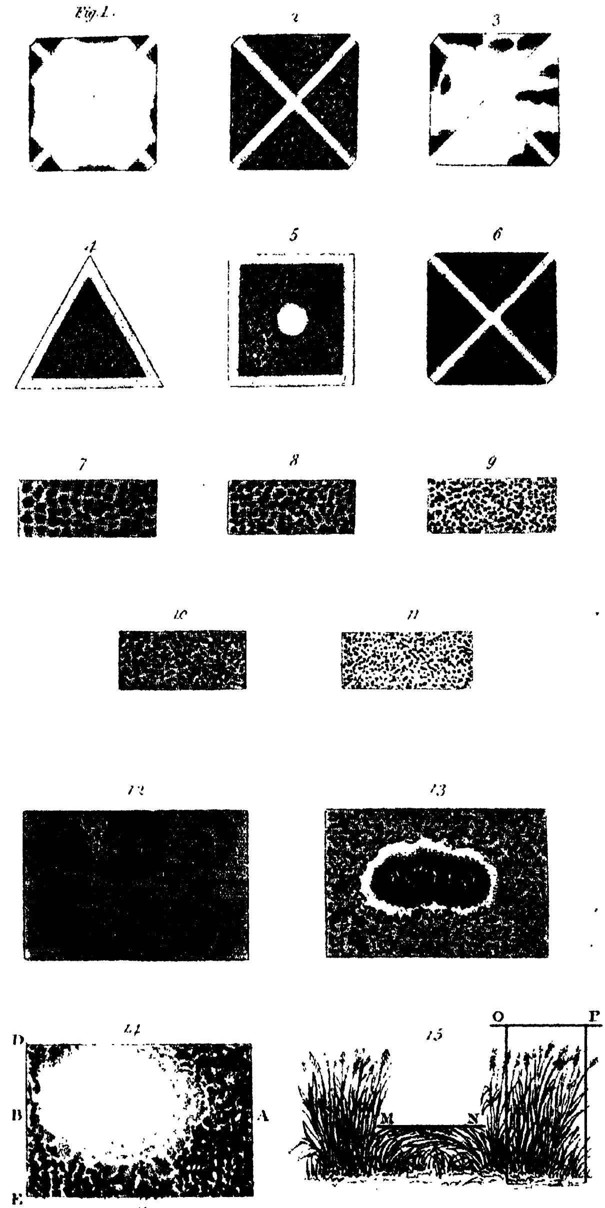
Fig. 1.
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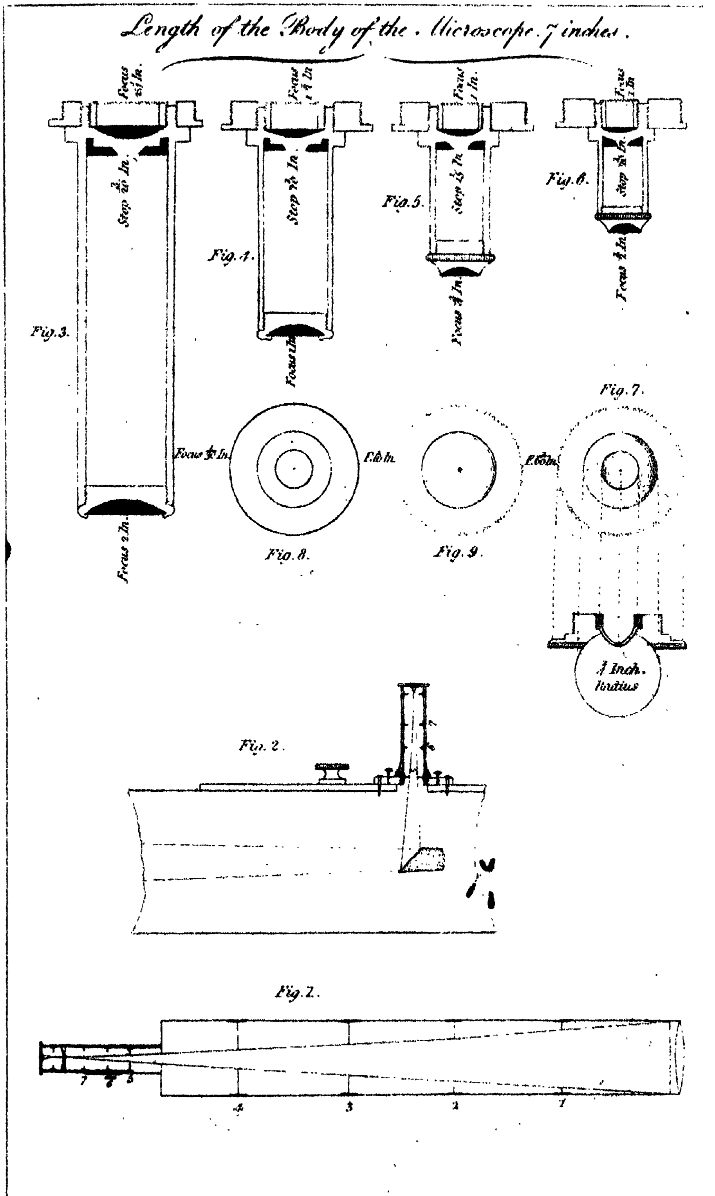
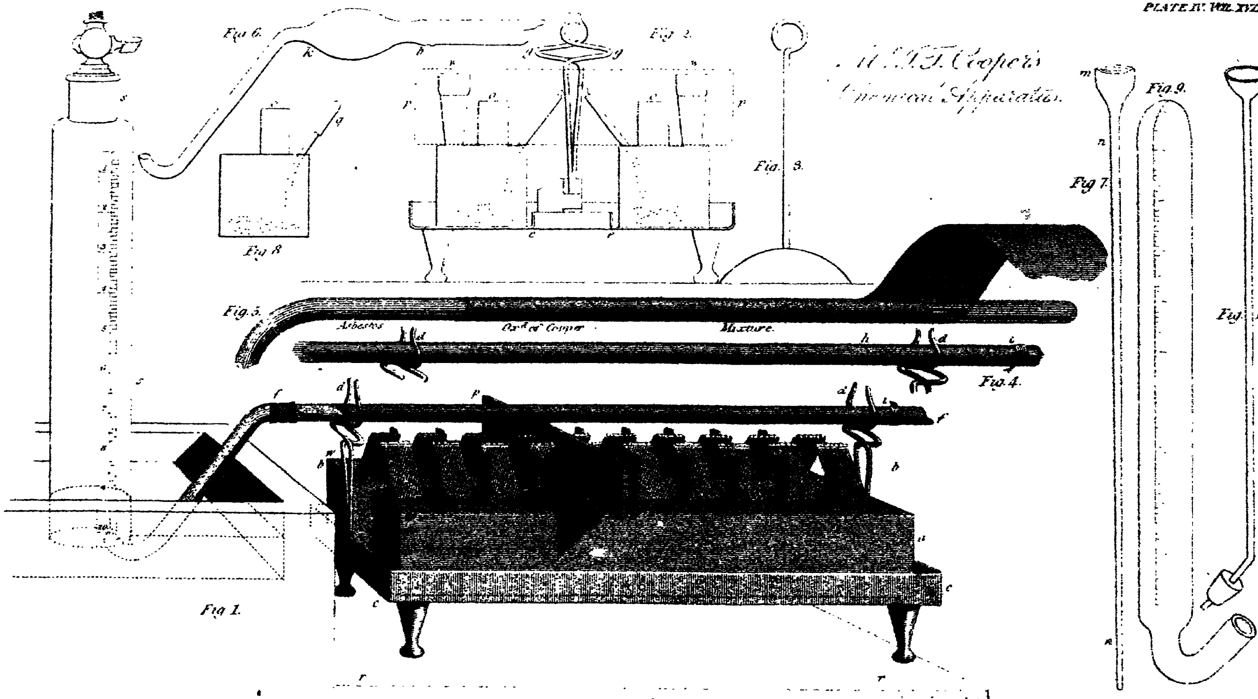


Js Basire sc.

Published by John Murray, Albemarle Street, London 1824.



C. Varley. Delt.

Published by John Murray, Albemarle Street, London, 1814.

James Basire Sculp.